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THE PSYCHOLOGICAL REVIEW

INTERVENING VARIABLE OR HYPOTHETICAL CONSTRUCT?

BY MELVIN H. MARX

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I

The improved scientific sophistication evidenced within recent years by psychological theorists has been largely characterized by an increased sensitivity to the need for operational validity in the formation and use of logical constructs. It has also become increasingly apparent, however, that operational validity, in and of itself, provides no guarantee for effective construction of psychological theory. As constructs are made progressively more operational, they must by definition be progressively divorced from the hypothetical content which seems to be regarded as a desirable if not essential component by a number of theorists. Within the past year both Tolman (27) and Krech (12), prominent theorists of so-called field-theoretical inclinations, have argued for the inclusion of a definite amount of hypothetical content in logical constructs. It is especially discouraging to find Tolman, whose introduction of the *intervening variable* (25, 26) contributed notably to the establishment of the recent operational trend, now apparently reversing his earlier position and stating that "to use Meehl and MacCorquodale's distinction, I would now abandon what they call pure 'intervening variables' for what they call 'hypothetical constructs,' and in-

sist that hypothetical constructs be parts of a more general hypothesized model or substrate" (27, p. 49).

One may certainly agree with Professor Tolman in his concern for the development of useful models. Nevertheless, it seems to be not only unnecessary but also distinctly dangerous to abandon the operationally defined intervening construct. It is the purpose of this paper, therefore, to attempt a clarification of the problem of the relationship between hypothesis and construct as these are used in contemporary psychological theory construction. The hypothetical construct and the intervening variable, to continue MacCorquodale and Meehl's (13) terminology, are regarded as lying on a single continuum, each type of construct having a certain useful function in the development of theory but the fully operational type remaining the ultimate theoretical objective. If psychological theories are to be placed on a sound scientific basis, logical constructs of the more distinctly operational type must first supplement and eventually replace those of the hypothetical construct type. However, it should be noted that there has probably been a tendency to overlook the value of constructs of the hypothetical type on the part of the more objectively oriented stimulus-response theorists, e.g., Hull (7, 8) and Spence (21, 22), as

well as an opposite tendency to minimize the value of the operational type on the part of the field-theorists. If the wholesale abandonment of operationally valid constructs is not required for the effective use of less operationally defined constructs, neither is the wholesale abandonment of the latter type required for the effective use of the former.

In attempting a clarification of this problem the present paper has a two-fold objective: first, to point out the *different* functions of each type of construct, and thereby justify their continued supplementary use; second, to describe a type of intervening variable which gives promise of offering an operationally sound alternative to the hypothetical construct but whose potential usefulness has not thus far been formally recognized.

II

Since the terminological distinction recently proposed by MacCorquodale and Meehl will be followed, a summary of their interpretation of the terms "intervening variable" and "hypothetical construct" will be useful. These authors distinguish between "constructs which merely abstract the empirical relationships (Tolman's original intervening variables) and those constructs which are 'hypothetical' (*i.e.*, involve the supposition of entities or processes not among the observed)" (13, p. 106-107). They then summarize three characteristics whose presence is typically indicative of the purely abstractive kind of construct—the "intervening variable," as they propose to call it—and whose absence is indicative of the "hypothetical construct." These characteristics are:

"First, the statement of such a concept does not contain any words which are not reducible to the empirical laws. Second, the validity of the empirical laws is both

necessary and sufficient for the 'correctness' of the statements about the concept. Third, the quantitative expression of the concept can be obtained without mediate inference by suitable groupings of terms in the quantitative empirical laws" (13, p. 107).

In the present discussion, a somewhat less detailed differentiation is required between the two terms, although the general tenor of MacCorquodale and Meehl's distinction is retained. By 'intervening variable' is meant any intervening construct with a maximum amount of operational validity, or direct empirical reference, and by 'hypothetical construct' is meant any construct with a relatively low degree of operational validity. These two terms, while specifically applicable to behavior problems, may thus be related to a wider methodological framework.

It is also necessary to recognize that a clear-cut distinction cannot always be drawn, in actual practice, between these two "types" of constructs. Any such impression, based upon the treatment by MacCorquodale and Meehl or the following discussion, should be quickly discouraged. For the sake of convenience in exposition, however, their essential continuity will not generally be emphasized in the following sections.

III

We may begin our consideration of this problem with a brief examination of the origin and development of logical constructs. A series of stages in this development will be roughly classified and briefly described.

1. *Pre-scientific origins.* From an historical point of view, logical constructs, like scientific problems and hypotheses, originate in the common-sense reflections of men. At a somewhat more sophisticated stage of concept-development various refinements may be made which tend to sharpen the dictionary

definitions of concepts and thus to increase the degree of verbal appeal, but which do little to tighten the relations between the concept and the relatively uncontrolled observations which produce it. Such refinements are typically made in philosophy, theology, politics and similar fields. Distinct and dogmatic biases in some of these fields often provide a certain consistency to conceptualizations but scarcely improve their operational validity. Little interest is shown in operational definitions.

2. Preliminary scientific formulations.

As objective scientific interests arise in a special subject-matter area two basically conflicting needs, which may be seen as fundamental to the subsequent conflict between hypothetical content and operational validity in constructs, become apparent. On the one hand, there is the immediate need for a scientific formulation of problems. Old questions have to be reworded in such a manner that relevant and controlled data can be collected. On the other hand, there is also the increasing pressure for continued operational refinement of the conceptual system that has been taken over, by and large, from the grossly non-operational systems of the earlier pre-scientific periods. The result is that the pioneering scientist is typically forced to compromise—that is, to brush up his conceptualizations, at least making some gestures in the direction of more operational validity, but actually retaining a certain significant degree of hypothetical content.

a. General role of hypothetical constructs. Let us examine more closely the role of the hypothetical construct in the preliminary phases of a new scientific development, such as has been represented within the past few decades by psychology. Of the host of concomitant problems facing the scientist none is more important than the need to ask, as simply and objectively as possible, the

kind of straightforward questions which can be given direct empirical answers. Now such questions, it should be apparent, are not easily discovered—not, that is, if they are to have that systematic usefulness that is required of any comprehensive scientific development. In all phases of science such empirical questions must be derived from some kind of prior hypotheses (*cf.* 1, Chap. 11). In the framing of these hypotheses the scientist must of course use whatever conceptual materials are at hand. In the absence of previous operational refinements he will be forced to rely, in a manner that ought to be explicitly recognized as a temporary expedient, upon such operationally inadequate conceptualizations as we are calling hypothetical constructs. How quickly such constructs can be replaced by more operational ones will of course depend upon a number of complex factors, among which may be mentioned the difficulty of the specific subject matter, the ingenuity and scientific skills of the investigators, etc. It is fairly obvious that large differences of this kind have thus far accounted for the appreciable variations in scientific development among the divisions of behavior study.

b. Problems of semantic usage. Effective scientific construct formation is complicated by certain unavoidable terminological difficulties. Although these operate at all stages of scientific development, they are most apparent, and probably most serious, during the early phases.

It is important to recognize at the outset that this semantic problem is actually distinct from the problem of operational validity, although the two are easily and commonly confused in practice. That is to say, the question of operational validity concerns simply the problem of relating constructs, however they may be named, to the particular

empirical data from which they were derived (*cf.* 14, 18). We are now concerned with the independent problem of choosing names for constructs which, whatever degree of operational validity may be present, will most effectively express their observational basis as well as their theoretical implications to those other persons with whom the scientist wishes to communicate.

An immediate problem is posed, as new constructs are introduced or old ones modified, by the necessity of either (1) using old words, or other symbols, which ordinarily already have acquired a variety of vaguely overlapping meanings, or (2) coining new words or other symbols, which may carry relatively unambiguous meaning but are seldom received with much enthusiasm by those outside the immediate systematic framework within which the new concept was developed.¹ In neither case, unfortunately, is there likely to be a long-lasting clarity of meaning achieved, since in spite of the best precautions each reader tends to read into the word, and to a lesser extent perhaps the other types of symbols, his own meanings and biases. This tendency then becomes more difficult to avoid as the term is circulated more widely, perhaps gaining in popularity and thus being increasingly related to other, less carefully defined conceptualizations. The use of isolated letters or mathematical symbols, exemplified by Hull's systematic behavior theory (7, 9), does appear to have the advantage of at least reducing the rate of such popular contamination.

From the standpoint of the subsequent user of the various symbols that represent scientific concepts, a sugges-

tion by Maslow (16) seems to merit serious consideration. He has proposed the formal use of what many of us probably tend to do more or less implicitly and informally—namely, the appending of a subscript, consisting of the original author's name or initial, to indicate the specific meaning that is intended by the use of the particular symbols.

This technique carries the disadvantage of being too awkward for common usage and, more important, of having to contend with the frequent variability that occurs within the usage of particular terms from time to time by the same author. Nevertheless, it has the very important advantage of recognizing not only the great variety of different meanings invariably acquired by common words but also the tendency, if not the privilege, which in actuality writers have of using words in ways that suit their own particular purposes. The common failure to appreciate this latter fact is a particularly unfortunate source of confusion. For example, the question, "What is perception?" (or "cognition," or "learning," etc.) is a type which is all too frequently found, either formally stated or implied.² Or, to use somewhat more sophisticated examples, the questions as to whether "fear" is distinct from "anxiety," "conflict" from "frustration," etc., tend to overlook the simple fact that authors may use these words in such a variety of ways as to make possible practically any answer they may wish. Explicit recognition of this fact and more careful attention, as a result, to the ob-

² It may be helpful to enumerate some of the more obvious answers to this kind of a question. The concept "perception," for example, may refer to (1) a field of study, (2) a set of more or less specific motor responses, (3) certain physiological processes, (4) a kind of subjective experience, (5) an intervening variable (or hypothetical construct). Which of these is meant must of course be determined if possible from the context.

¹ A modification of these two methods which seems to have certain advantages may also be mentioned. This consists of using familiar words but in a new grouping. Examples would be Skinner's *reflex reserve* (20), Hull's *fractional anticipatory goal reaction* (5, 6) and Hovland's *inhibition of reinforcement* (4).

servational determinants of such shift words would certainly save a considerable amount of the type of discussion and argumentation that helps to keep a large part of psychological theory on the "debating society" level of discourse.

3. *Advanced scientific analysis.* From the point of view of effective theory-construction, scientific advance may be considered to be a function, in large part, of the extent to which hypothetical constructs can be transformed into operationally purified intervening variables. In attacking this problem we may first consider the three major outcomes of the use, in preliminary scientific formulations, of those hypothetical constructs with which the scientist undertakes his early theoretical endeavors.

a. Continuation as hypothetical constructs. Unfortunately the most frequent outcome of the use of hypothetical constructs seems to be that they are simply retained as hypothetical constructs, often being modified through a process of more or less extensive verbal reorganization. Thus many of the psychoanalytic constructs have acquired, through a series of primarily verbal accretions, all sorts of alleged explanatory properties, with a progressive widening of the gap between them and specific empirical verifications. This process is well described by MacCorquodale and Meehl (13, p. 105 ff.). It is this outcome which is responsible for so much of the dissatisfaction on the part of many psychologists with the use of hypothetical constructs. The distinctly negative reactions produced by the more flagrant bad examples of such theorizing tend also to carry over to theory and theory-construction in general, as Spence (23) has observed. This unfortunate situation, by no means peculiar to psychology but perhaps for many reasons more prominent there, is the price that must be paid if the more desirable fruits of such activity are to be

enjoyed. Probably the only real solution is a continuing pressure on the users of constructs and the developers of theory to improve the operational validity of their formulations.

b. Suggestion of empirical research. The most obvious scientific values that derive from the use of hypothetical constructs are those that involve the suggestion of empirical, preferably experimental, research. As mentioned earlier, it is this particular function of the hypothetical construct which both Tolman (21) and Krech (12) have recently emphasized. In this respect exception must be taken to the conclusion of MacCorquodale and Meehl that "hypothetical constructs, unlike intervening variables, are inadmissible because they require the existence of entities and the occurrence of processes which cannot be seriously believed because of other knowledge" (13, p. 106). It is precisely this characteristic of enabling an investigator to go beyond present knowledge and not to be tied down to currently orthodox formulations (which may of course later be viewed as wholly inadequate) that helps to justify the usefulness of such hypothetical constructs as guides to experimentation. Köhler's investigation of the figural after-effect and related perceptual phenomena (10, 11) is an excellent example of this point. The only valid basis on which such hypothetical constructs may be rejected involves their failure to lead to adequate empirical tests.

Although one must recognize the potential fruitfulness of the use of such models as may be generated by means of hypothetical constructs, it is necessary to exercise considerable caution with regard to the conclusions which are drawn from empirical results obtained in this way. If the model is regarded only as a tool to be used in suggesting empirical investigation and then discarded, or modified and retained as

a useful guide to experimentation, little danger of unjustified theoretical interpretation is likely to be present. More commonly, however, there is a tendency to regard the empirical results obtained as supporting, in some way, the *theoretical* validity of the original model and the particular hypothetical constructs which have been used.⁴ Such conclusions cannot be legitimately drawn, and, as a matter of fact, should be quickly and vigorously labelled as inadmissible *unless* in the process of investigation and theoretical reformulation the constructs have acquired a more adequate degree of operational validity (that is, have moved significantly in the direction of true intervening variables).

That some disagreement on this point exists, is, I think, fairly obvious. For example, Krech writes as follows: "Because it is assumed that these hypothetical constructs exist, and because of the extrinsic properties that they are assumed to have, the correlations between experimental conditions and results are now seen as *necessary* correlations, as inevitable consequences of the functioning of these hypothetical constructs" (12, p. 75). Now, it is essential to note that if such "postulated actually existing structure" is finally reduced to direct experimental measurement and purely empirical statements, it can no longer be regarded, in any useful sense, as a *construct*. And if it continues to be defined indirectly through experimental measurement, and thus remains a construct, any *necessary* correlations of the kind Krech indicates can result only if operational refinement appreciably reduces the original ambiguity of the construct and it thus moves in the direction of the intervening variable.

In clarification of this point it may be helpful to make a gross qualitative distinction between two types of hypothetical constructs: those which postulate the existence of some specific entity or

process, the direct empirical identification of which is regarded as a major objective; and those which are deliberately designed to serve only as constructs and thus do not elicit attempts at direct empirical identification. Operational validation of both types is of course possible. As noted above, this leads in the case of the first type to purely empirical propositions, and in the case of the second type to the intervening-variable kind of construct. It must be recognized, however, that in actual practice the scientific usefulness of the first type does not depend upon its "existence" in a specific empirical sense. An unfortunate amount of confusion seems to have been produced by the somewhat naive expectation that certain hypothetical constructs, for example, the gene, may some day be "seen," in a literal sense, through some sort of direct and specific identification of the entity or process "as it really is." Whether or not this is ever done (and parenthetically it may be observed that most such claims are to be regarded as highly tenuous, at best), it may be said that, from a more realistic point of view, all that need be expected in the way of direct empirical identification is the development of a series of progressively more refined empirical propositions. These propositions typically bear only the slightest resemblance to the originally postulated construct.

The necessity for operational validity in scientific constructs is in no way reduced if the development of a system of formal models is regarded as the primary objective of science rather than merely as a useful device to direct and unify empirical investigations. For example, Rosenblueth and Wiener, who adopt the former position, nevertheless also state that "The successive addition of . . . variables leads to gradually more elaborate theoretical models; hence, to a hierarchy in these models,

from relatively simple, highly abstract ones, to more complex, *more concrete* theoretical structures" (19, p. 319, italics added). It would certainly seem that the only way in which theoretical structures can be made "more concrete" is through coordinating empirical measurements of the kind that characterize operationally valid constructs.

It may be suggested that a large amount of the apparent confusion in psychology on the role of such theoretical structures has been due to the tendency to think in terms of the way in which they have been employed, with eminent success, in the physical sciences. The potential usefulness of theoretical structures of so high a degree of abstraction need not be denied. Nevertheless the most immediate need of psychology would rather plainly seem to be the careful development of a large number of low-level empirical laws and low-order theories based upon the use of intervening constructs of the more operational type. Higher-order theoretical generalizations may then be built upon a sound empirical framework, in accordance with orthodox scientific procedure (*cf.* 2, 19) rather than developed from above as has too often been attempted in the past, and is encouraged by the premature emulation of theoretical models based upon the highly abstract physical pattern.

Support for this point of view is readily adduced from consideration of previous attempts in psychology to construct elaborate theoretical models. With regard to Freudian psychoanalysis, for example, it is instructive to note the generally more favorable reception by academic psychologists of those constructs such as repression and other so-called "dynamisms" which involve relatively close functional relations to empirical operations than those high up in the theoretical superstructure, such as id, ego and super-ego, which bear only

remote and tenuous relations to their observational bases. The extent to which any such theoretical superstructures have actually contributed to the development of psychology in a sound scientific direction may be seriously questioned. Their disadvantages are obvious. Broad systematic frameworks certainly serve some useful functions, but in the present state of psychology the most valuable formal models, like the most valuable experiments, seem to be those with definitely restricted objectives.

c. Transformation into intervening variables. As has been emphasized throughout the discussion, the ultimate objective of all theoretical construct formation is believed to be the operationally valid, intervening-variable type of construct. Although no hard and fast rules can be easily prescribed for this important transformation and no completely black and white distinctions can be made, a few of the more obvious factors may be mentioned. In general, it is essential that the operationally inclined theorist think in terms of *experimental* procedures as well as theoretical structures. One of the most common characteristics of those theorists who are inclined to rest content with hypothetical constructs is their general tendency to be concerned with verbal distinctions and their general neglect of a critical analysis of the observational bases of their conceptual framework. A second factor which encourages the early transformation of hypothetical constructs into intervening variables is the tendency to set up definite, more or less *formal hypotheses*. Obviously, the more hypothetical content that can be placed into formal hypotheses the less need there will be for including it in the constructs that are used.³

³ In this respect it is interesting to note that Tolman's major point in favor of use of hypothetical constructs, rather than the intervening

Thirdly, the operationally inclined theorist will make a deliberate effort to set up hypotheses which are susceptible to more or less direct *empirical test*, and to work out the implications of the hypotheses that can be so tested. This means that he is more likely to be concerned with specific than with general problems. The operationally disinclined theorist, on the other hand, often tends to favor hypotheses which are either themselves relatively untestable or whose empirical consequences are not easily determined. A further characteristic of such hypotheses may be seen in the fact that it is frequently difficult if not impossible to refute them. Finally, the operationally inclined theorist is generally more interested in the problem of *quantification*, while the operationally disinclined type of theorist is more often content with purely qualitative distinctions.

Before leaving this general problem of modification of constructs, it may be suggested that too rapid an attempted transformation into purely operational intervening variables seems to have certain disadvantages. The major risk of such over-expansion is that associated with the premature development of an overly rigid systematic position, especially if there is a concomitant attempt to stifle continued exploratory efforts at other levels of explanation. The growth of such rigid positions into cult-like systems, with blind acceptance of certain key principles, serves to offset the gen-

variables with "merely operational meaning," is that the latter "really can give us no help unless we can also imbed them in a model from whose attributed properties we can deduce new relationships to be looked for" (27, p. 49). This, of course, is exactly what Hull's systematic behavior theory (7, 9) has attempted to do, largely without the use of hypothetical constructs—although Tolman would probably prefer to use a "model" other than the one Hull has fashioned.

unely sound scientific advances that can result from such concentrated research programs.

IV

Two particular types of intervening variables, each of which seems to have a distinct function in the development of sound scientific theory in psychology, will now be discussed.

1. *The orthodox type of intervening variable.* As ordinarily conceived, the intervening variable is a kind of shorthand expression for the performance of certain specific empirical operations. This type of construct often but not necessarily (*cf.* 20, 26) has been given a strong quantitative flavor and placed in a highly mathematical framework (*cf.* especially 7 and 9). The advantages of such a conceptual tool have been well described in many other papers (*e.g.*, 7, 8, 13, 14, 21, 22, 25) and need not be reviewed in detail here. Hull's systematic behavior theory may be considered a good example of the potential value of this use of intervening variables. As even certain of his critics have said (*e.g.*, 3), the development of this type of rigorous scientific system marks a definite objective toward which psychological theories should in the future point.

2. *The E/C type of intervening variable.* I should now like to call attention to a somewhat different use of the intervening variable technique which has been occasionally employed but apparently without formal recognition, and which I think offers considerable promise. Like the orthodox type of intervening variable, this type is a shorthand expression which represents the performance of certain specified empirical operations. Although sharing the generally desirable operational characteristics of the orthodox type it does in addition have the peculiar advantage of

contributing to the semantic clarification of psychological language.

In essence, this usage simply provides a particular name—representing the postulated intervening variable—to account for whatever specified behavioral differences are empirically found to result from a specified set of stimulus operations. Since a construct of this kind must generally be a function of a comparison between experimental and control conditions, the term E/C is suggested to mark it off from the orthodox usage. It may best be illustrated by an example from the experimental literature.

In a recent study by Mowrer and Viek (17) hungry rats were given the opportunity to eat moist mash from the end of a small stick offered them through the bars of a shock grid. An electric shock was delivered 10 seconds after they had begun to eat, or following an additional 10-second interval if no eating occurred within the first 10 seconds. The experimental animals were able to turn off the shock by leaping off the grid, an act which they quickly learned to perform. The control animals were unable to influence the shock directly, but each control animal was given exactly the same duration of shock on every trial as its matched experimental animal. Under these conditions experimental and control animals received identical amounts of electric shock, as objectively measured, but there was an important behavioral difference. The experimental animals ate significantly more frequently and more quickly than the controls. The "sense of helplessness," which is the verbal tag that Mowrer and Viek gave to the assumed psychological function more consistently present in the control than in the experimental animals, may be regarded as a true intervening-variable type of construct since it can be tied

down, on both the stimulus and the response sides, to empirical measurements.⁴

The semantic advantages of this use of the intervening variable should be apparent. When we speak of any E/C intervening variable we mean—or should mean—nothing more than whatever intervening function needs to be assumed in order to account for the experimental-control differences empirically observed. However, in deciding which verbal label to give this intervening variable, we may draw upon our own informal observations or upon some particular theoretical framework. This use of the intervening-variable technique thus makes it possible not only to give a purely operational meaning to the constructs used, but also to relate them to some prior observations or theoretical system in a way that should help to move these constructs in the direction of a more clear-cut operationism.

There is an obvious semantic danger in this process, however, which needs to be clearly faced. It is most apparent when the names chosen to represent the intervening variables have otherwise acquired a large number of vague and varied meanings. The danger is that such relationships will be emphasized and that subsequent investigation of the basic behavioral functions will be correspondingly diverted. Use of relatively neutral symbols, like letters of the alphabet, may be helpful in discouraging this kind of verbal regression, but these seem to be more readily applied in connection with the orthodox type of intervening variable.

⁴ It should be noted that this is true in spite of any doubts that may be entertained concerning the particular term chosen and the further theoretical implications suggested. In the present case, for example, I have elsewhere (15) questioned Mowrer and Viek's choice of the term "sense of helplessness" on the ground that their theoretical interpretation of the experiment is not adequately supported.

The problem of the generality of intervening variables of the E/C type also needs to be considered. If concepts of a high degree of generality are essential objectives of scientific theory, how can they be obtained through the use of constructs postulated specifically to refer to a particular set of experimental operations?

Two answers to this question may be suggested. In the first place any particular construct can be given strict, operational meaning through the E/C technique, and can subsequently be broadened to refer to an increasing number of different kinds of experimental situations. Such a broadening of meaning, or generalization, must of course be done with considerable caution. The great advantage of using successive E/C situations, however, is that if anyone cares to question such a generalized construct, he may refer directly to the identifying experiments. The degree to which any given construct can be thus generalized will largely depend upon the extent to which the specific empirical situations upon which it is based may be related. In this respect it is useful to recall Stevens' solution for the basic problem of generality in operationism, which may be summarized in his statement that "We combine operations when they satisfy the criteria of a class; and the concept of that class is defined by the operations which determine inclusion within the class" (24, p. 234).

The controlled manipulation of experimental designs upon which successful development of such generalized concepts will depend must be recognized as a difficult but by no means impossible task. Moreover it is one which psychologists must somehow effectively tackle if they intend to improve the scientific systematization of their theoretical frameworks. As a simple but concrete example of the manner in

which related experimentation may in actual practice be performed, mention may be made of a modification of the Mowrer and Viek design, emphasizing a so-called "sense of control," which has recently been used by Marx and Van Spankeren (15) in a study demonstrating a certain amount of learned "control" of the audiogenic seizure by rats. This construct is definitely of the intervening-variable type since it has been given a thoroughly operational meaning, in spite of its subjective sound, in terms of the E/C differences.

Consideration of the relationship between the E/C type of intervening variable and the orthodox type provides us with a clue to a second solution of the problem of generality. E/C intervening variables are considered to be most useful in the exploratory phases of scientific theory construction and experimentation, as a means of spotting new variables, probing for gross functions, etc. As experimental investigation progresses the E/C variables should ultimately be reducible to the more general orthodox type, and in fact should aid greatly in the discovery and delimitation of these in the later stages of systematic theory construction. Translation of such complex variables as "sense of control" into relatively more abstract and general constructs may then be expected to result from their continued experimental analysis. From this point of view the essential continuity of the E/C and the orthodox types of intervening variables is evident.

The other major advantages of the E/C usage of the intervening variable may now be briefly summarized. In the first place, and most importantly, it provides a technique which seems to combine the best features of both the hypothetical construct and the orthodox intervening variable. That is to say, it offers the experimenter an opportunity to draw upon the suggestions of a

theoretical model and yet remain on a strictly operational level of discourse. It thus provides a high degree of freedom of experimental investigation without sacrifice of the methodological rigor that normally accompanies the use of the orthodox intervening variable. This is an especially important characteristic in the present stage of psychological science, with the concomitant needs for more exploratory work and the careful identification of the empirical bases of constructs. Secondly, it permits the formal separation of the hypothetical from the conceptual components of theory construction, and requires that the investigator think more in experimental and less in purely verbal terms. Thirdly, by enabling the empirically minded investigator to indulge, cautiously, in a small amount of construct formation, it encourages the development of a greater amount of theoretical orientation on the part of psychologists whose antipathy to theory and theory construction largely stems from their distrust of hypothetical constructs and accompanying speculation. An increased interest in theory construction by this kind of investigator is regarded as a highly desirable objective.

The successful use of the intervening variable of the E/C type involves several important requirements. It obviously depends, in common with all sound scientific work, upon the experimental validity of the empirical data. It also necessitates a certain amount of ingenuity in the design of the experiment, but once the proper theoretical-experimental attitude is acquired this may be less difficult than it at first appears. It demands that the investigator, once he has performed an experiment and defined his intervening constructs purely in terms of his experimental operations, now continue to apply those particular conceptualizations in an operationally sound manner as

he attempts to relate them to the wider theoretical framework from which they have been derived. This means that, as implied earlier, a clear line needs to be drawn between theoretical implications that are to be directly made upon the basis of the experimental results and those that are merely suggested by them. Finally, it requires that the investigator think in terms of narrow, experimentally manipulable problems—even if they are imbedded in a significantly wider theoretical framework. This is an especially desirable requirement, since it should result in an improved experimental sophistication on the part of those whose speculations too often tend to outstrip their empirical foundations.

In conclusion it may be noted that there is in this discussion no intention to imply that the E/C usage represents anything more than a refinement and a formalization of currently accepted scientific procedure. However, it is hoped that this technique will encourage that active *experimental* search for new variables and new relationships between variables which is essential for the continued scientific advancement of psychology.

V

In summary, the following conclusions are offered:

1. The hypothetical construct, as defined by MacCorquodale and Meehl (13) and recently justified by Tolman (27) and Krech (12), is to be regarded in general as a temporary expedient in the development of sound psychological theory.

2. Hypothetical constructs are most useful in the early, preliminary phases of scientific work. Their use may have three major outcomes: (a) They may be continued, perhaps in modified form, on a grossly non-operational level of discourse. This practice cannot be sci-

entifically defended. (b) They may lead to important empirical investigations, in which case they serve a useful function in suggesting research. However, the empirical results must not be regarded as constituting evidence in support of the theoretical validity of such conceptual models unless in the process they are given increased operational validity. (c) They may be transformed into operationally valid intervening variables, which are the only kinds of constructs ultimately admissible in sound scientific theory.

3. Two types of intervening variables are currently being used in psychology, each with an important function: (a) The orthodox type of intervening variable is simply a shorthand symbolic expression, often quantitative in character, of a specified set of experimental operations. It is necessary in the relatively advanced stages of theory construction. (b) The E/C type of intervening variable is likewise a verbal expression of a specified set of experimental operations, but is more directly related to the experimental-control differences in the experiment. It offers, through the method of successive approximation, an opportunity to clarify semantic usage in psychological theory construction. Its relatively greater flexibility as a methodological tool makes it especially useful in the exploratory stages of scientific investigation when theoretical constructs need to be progressively released from their pre-scientific ambiguity. It is thus regarded as an operationally valid alternative to the hypothetical construct.

4. It is strongly recommended that psychological investigators pay more attention to the formal operational requirements of their theory construction, and, in particular, attempt more explicit use of both types of intervening variables.

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INTERRUPTION AND LEARNING

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I. INTRODUCTION AND POSTULATES

The publication of Zeigarnik's study (7) has led to a variety of experiments designed to test the effect of interruption on recall. For a review of the literature the reader is referred to the summaries and competent evaluations by Prentice (5) and McGeoch (4, pp. 383-386).

The heuristic effect of Zeigarnik's contribution may be attributed, among other things, to her (a) methodology, (b) theoretical interpretation, and (c) statement of conditions which affect the mnemonic advantage of interrupted tasks. By far the greater portion of investigation has been concerned with the last factor. Some authors have tacitly accepted Zeigarnik's theory; others fitted the results into other frameworks. The methodology, however, which largely accounts for Zeigarnik's findings has not been thoroughly examined. Some years ago I presented before a convention of the American Psychological Association a study by Boguslavsky and Guthrie (1) which contained a criticism of Zeigarnik's methodology and of her theoretical interpretation. This criticism, together with the experimental findings and a suggestion for a new approach to the problem, is presented in the pages to follow.

Most psychologists will agree, no doubt, that the process of interrupting a task constitutes an introduction of new factors in the stimulus situation. It is surprising, therefore, that so little effort has been made to observe the behavior of the organism at the time this change occurs. It has been assumed almost universally that the only responses

which merited observation were the ones that took place at recall. The temporal gap between the stimulus of interruption and the response of recall has been filled with intervening variables, such as psychic tensions, which are neither observable nor meet the criterion of hypothetical constructs as outlined by Hull (3, p. 22) because of the vague manner in which the relationships are stated. Another attribute of these intervening variables is that they are affected by the personality characteristics of the subjects. The latter, however, are response-derived constructs and can lead only to what Spence (6) has called $R_1 = f(R_2)$ types of law, often useful in prediction, but sterile from the point of view of control.

Freeman's (2) approach to the problem is a happy exception. Not satisfied with Zeigarnik's explanation, he observed the muscular activity of his subjects throughout the experiment. His results indicate that the interruption of a task uniformly produces an increase in the muscle tonus, which tends to decrease with the passage of time. Another observation made by Freeman is that an increase in tension also takes place at the beginning of each new task, and this, too, subsides gradually. Although Freeman investigated a relatively circumscribed muscular area, we may safely infer that the nature of the change observed by him is not restricted to that area but is indicative of a more general neuro-muscular transformation. The relation between this transformation and the recall of tasks is discussed below.

The recall of a task is a learned verbal response, given by a subject con-

fronted with a certain environment, external and internal. Obviously the learning of this response takes place while the subject is engaged in the task. Thus the performance of each task may be considered as a separate learning situation during which a specific verbal response becomes attached to a specific combination of stimuli. If this analysis is accepted, the relation of muscular tension to recall becomes clearer. McGeoch's (4, p. 287) summary of experimental evidence on the subject indicates that, within certain limits, tension is positively correlated with rate of learning. Hence, if these limits are not exceeded, tasks accompanied by greater tensions will be learned and recalled more frequently than tasks accompanied by lesser tensions.

The following postulates are an attempt at a quantitative description of the learning process in an interruption experiment:

Postulate 1. When a subject is interrupted in a task, or is confronted with a new task, he responds with a general neuro-muscular transformation, some aspects of which are directly measurable as an increase in the state of bodily tension. This increase is a function of the manner in which interruption or presentation takes place, and it varies from subject to subject.

Postulate 2. The state of tension tends to subside as the task progresses. The rate of subsiding is a function of the nature of the task, and it varies from subject to subject.

Postulate 3. The initial state of tension and the rate at which it subsides are represented by the function $y = se^{-ts}$.

Postulate 4. The parameter s and the definite integral of the function within certain limits have greater positive values when tension is occasioned by interruption than when it is occa-

sioned by the presentation of a new task. The maximum upper limit of the integral varies from subject to subject, with the average maximum postulated at about 45 seconds.

Postulate 5. Exaggerated action produced by the state of increased tension generates volleys of movement-produced stimuli. These, if occurring contiguously with the performance of a task, become cues for the verbal responses connected with the task.

Postulate 6. The probability of a task's occurrence in recall is a direct linear function of the amount of movement-produced stimulation associated with the task. This linear function is the same for all tasks within the same experimental situation.

Postulate 7. The probability rate of the incidence of movement-produced stimulation during the performance of a task is represented by the function $y = ae^{-bs}$, in which the parameters a and b are linearly related to the parameters s and t of Postulate 3.

II. METHODOLOGICAL ANALYSIS

One implication of the foregoing postulates is that the effects of interruption are not limited to the task which is interrupted, but may extend to any activity which immediately follows interruption. In this connection I shall take up three types of experimental procedure leading to three different types of results.

Procedure 1: Interruption of a task is simultaneous with the presentation of a new task to which the subject responds at once.

This situation is illustrated in Fig. 1. The four curves represent the rate of incidence of movement-produced stimuli under the following conditions:

A. The subject was permitted to complete the preceding task as well as the present task.

B. The subject was permitted to complete the preceding task, but is interrupted in the present task.

C. The subject was interrupted in the preceding task, but is permitted to complete the present task.

D. The subject was interrupted in the preceding task as well as in the present task.

The shaded areas under the curves represent the amounts of stimulation associated with the task when it appears under each of the four conditions. From Postulates 4 and 7, the initial ordinate and the area at *C* are greater than those at *A*. Similarly, these values at *D* are greater than those at *B*. Hence, from Postulate 6, we should expect that tasks performed under conditions *C* and *D* (i.e., preceded by interruption) will be recalled more frequently than tasks performed under conditions *A* and *B* (i.e., preceded by completion).

Also, because of a difference in time between completed and interrupted

tasks, the area at *A* is greater than that at *B*, and the area at *C* is greater than that at *D*. Hence, with this experimental procedure, we should expect completed tasks to be recalled more frequently than interrupted tasks.

The accuracy of both predictions is tentatively corroborated by the results of an experiment which was part of the study by Boguslavsky and Guthrie (1). Because that study has previously been published only as an abstract of the report, the experiment is described in some detail below.

Eighty college students served as subjects, and each was given a set of 20 tasks. Each of these was a paper-and-pencil task, performed on a $3\frac{1}{2} \times 8\frac{1}{2}$ " slip of paper. The average time required for completion was 32 seconds. Instructions for each task were printed at the top of the paper, making oral communication unnecessary. The subject was allowed to carry 10 of these tasks through to completion, but in the other 10 he was interrupted shortly before the end by having the task removed and a new one presented in its place. The average time spent by the subject on an interrupted task was 24 seconds. The completed as well as the interrupted tasks were evenly divided into those preceded by completion and those preceded by interruption. Four different orders of presentation were used, so that each task appeared an equal number of times under each of the conditions *A*, *B*, *C*, and *D*, described above. At the end of the series the subject was asked to name the tasks on which he worked, in any order he chose.

The results and their statistical treatment are presented in Table I. Each cell entry represents the number of times a particular task was recalled when it appeared under the specified condition. In computing the *t*-values the second order interaction was used as basis for estimating the population

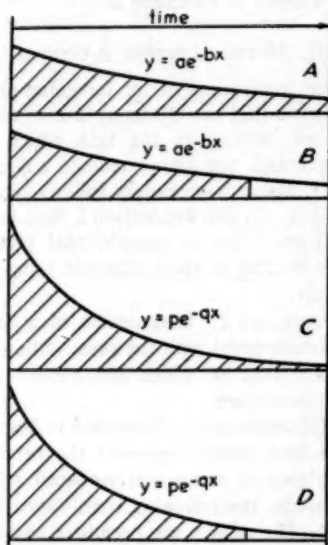


FIG. 1. Stimuli contiguous with performance under experimental Procedure I.

variance; this is the interaction between (a) tasks; (b) manner of presentation (preceded by completion *vs.* preceded by interruption); and (c) manner of termination (completed *vs.* interrupted). The *F*-ratios between the first and second order interactions were not significant.

The direction of results seems to justify both expectations, although only the first prediction is borne out with some degree of significance. Assuming, however, that the column totals of Table I are proportional to the true recall probabilities, it becomes possible, from Postulates 6 and 7, to compute the parameters *a*, *b*, *p*, and *q* of Fig. 1, as follows:

$$A. \int_0^{32} ae^{-bx} dx = 108k,$$

$$B. \int_0^{24} ae^{-bx} dx = 94k,$$

$$C. \int_0^{32} pe^{-qx} dx = 120k,$$

$$D. \int_0^{24} pe^{-qx} dx = 116k,$$

whence

$$a = .6558k,$$

$$b = .0474,$$

$$p = 15.054k,$$

$$q = .123.$$

Accepting these as approximations of the true parameters, and with the help of certain additional assumptions to be made, I shall endeavor to estimate the recall frequencies under experimental Procedures II and III.

Procedure II: Interruption of a task is simultaneous with the presentation of a new task, but the subject fails to respond to the new task immediately.

This situation is likely to occur when the experimenter gives oral instructions for a new task, at the same time that he withdraws the interrupted task. A

TABLE I
RECALL FREQUENCIES UNDER EXPERIMENTAL
PROCEDURE I

Tasks	Conditions of performance			
	A	B	C	D
1	9	9	10	13
2	8	7	5	7
3	7	10	10	9
4	10	10	4	6
5	7	4	7	6
6	5	2	9	4
7	2	1	1	1
8	1	2	2	3
9	10	4	14	11
10	3	4	8	6
11	3	2	5	6
12	3	5	5	6
13	1	2	1	2
14	1	0	0	0
15	1	1	1	0
16	10	7	6	11
17	1	5	5	2
18	6	4	8	6
19	12	10	12	9
20	8	5	7	8
Total	108	94	120	116

Difference in favor of tasks preceded by interruption: 34.

$$t = 2.361$$

$$P = .03$$

Difference in favor of completed tasks: 18.

$$t = 1.25$$

$$P = .23$$

change in response from visual to auditory stimulation is a change in postural set and receptor orientation. It takes place during the initial phase of the instructions and is incompatible with verbal responses to these instructions. Thus stimuli resulting from interruption are associated only in part with the new task, since the subject is engaged in something else while the incidence of stimulation is at its peak. The shaded areas in Fig. 2 represent stimuli which become cues for the task under Procedure II. The four conditions A, B, C, and D are those described earlier.

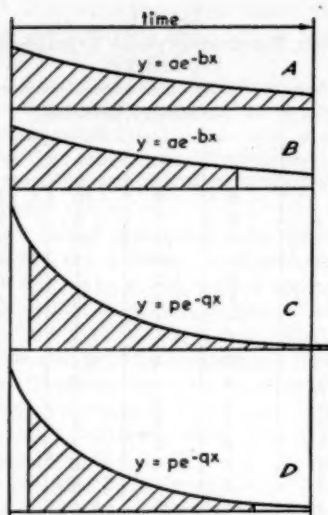


FIG. 2. Stimuli contiguous with performance under experimental Procedure II.

Assuming that the average time required for readjustment from visual to auditory stimulation is two seconds, and substituting the parameters computed under Procedure I in the new equations, we obtain the following predictions for recall under Procedure II:

$$A. \int_0^{32} 6.558ke^{-.0474x} dx = 108k,$$

$$B. \int_0^{24} 6.558ke^{-.0474x} dx = 94k,$$

$$C. \int_2^{34} 15.054ke^{-.123x} dx = 93.8k,$$

$$D. \int_2^{26} 15.054ke^{-.123x} dx = 90.7k.$$

An examination of the computed recall frequencies indicates a reversal of the first prediction made under Procedure I. Here the value at *C* is smaller than that at *A*, and the value at *D* is smaller than that at *B*. Since conditions *C* and *D* are those in which the

activity preceding the task had been interrupted, we should expect that, under Procedure II, tasks preceded by interruption should occur *less* frequently in recall than tasks preceded by completion.

With reference to the manner in which the task itself is terminated, our prediction is the same under Procedure II as under Procedure I. Since the value at *A* is greater than that at *B*, and the value at *C* is greater than that at *D*, we should again expect the completed tasks to be recalled more often than the interrupted tasks.

TABLE II
RECALL FREQUENCIES UNDER EXPERIMENTAL PROCEDURE II

Tasks	Conditions of performance			
	A	B	C	D
1	2	0	1	3
2	9	4	3	5
3	10	4	7	5
4	7	4	8	1
5	0	0	3	1
6	9	1	0	0
7	8	5	5	3
8	5	5	5	1
9	3	6	5	1
10	5	7	7	5
11	3	9	5	7
12	1	7	1	1
13	3	3	6	5
14	5	8	1	7
15	7	4	7	5
16	8	5	6	7
17	8	9	3	4
18	9	2	6	8
19	1	1	5	3
20	1	1	5	4
Total	104	85	89	76

Difference in favor of tasks preceded by completion: 24.

$$t = 1.155$$

$$P = .26$$

Difference in favor of completed tasks: 32.

$$t = 1.540$$

$$P = .14$$

The results of another experiment by Boguslavsky and Guthrie (1), though not sufficiently significant to warrant the rejection of the null hypothesis, nevertheless show differences in the predicted direction. In this experiment 40 students served as subjects. The tasks were of similar length and difficulty to those of the experiment described earlier in the paper. The only important variation was that the instructions for each task were given orally. The results are summarized in Table II.

Procedure III: Interruption of a task fails to terminate the subject's preoccupation with it, and the presentation of a new task occurs some time after the termination of interrupted activity.

This situation apparently existed in Zeigarnik's original experiment. According to her description, interruption was often countered with protest and with refusal to give up the task. Consequently the tension induced by interruption was permitted to accompany activities that should have been terminated.

Another important methodological aspect of Zeigarnik's experiment may be found in the time interval during which she removed materials connected with one task and laid out materials for the next task. For the subject this interval was filled apparently with irrelevant pursuits during which tension induced by interruption was dissipated. If the last assumption is correct, it follows that no task bore the effects of what had happened to the preceding task; and that the curve of incidence of movement-produced stimulation at the beginning of each task is represented by the parameters a and b . Figure 3 illustrates the experimental Procedure III, with the tasks performed under the four conditions A, B, C, and D.

For a quantitative analysis of Zeigarnik's experiment it is necessary to make assumptions with reference to certain

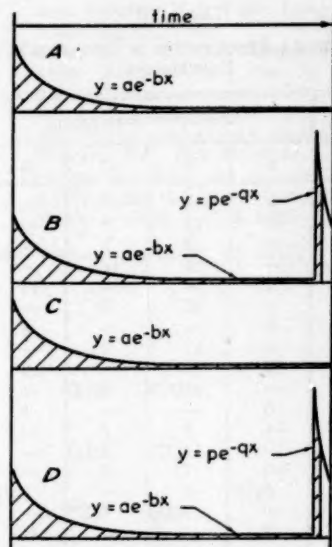


FIG. 3. Stimuli contiguous with performance under experimental Procedure III.

time intervals. Zeigarnik reports that most of her tasks lasted from 3 to 5 minutes, and that only a few lasted from 1 to 2 minutes, or less. Assuming that (a) the average time spent in completing a task was 200 seconds, (b) the average time spent on an interrupted task was 190 seconds, and (c) the average time between interruption and termination of that task was 4 seconds, we substitute for the parameters values computed under the experimental Procedure I, with the following results:

$$\begin{aligned}
 A. \quad & \int_0^{200} 6.558ke^{-.0474x} dx = 138.3k, \\
 B. \quad & \int_0^{190} 6.558ke^{-.0474x} dx \\
 & + \int_0^4 15.054ke^{-.123x} dx = 185.9k.
 \end{aligned}$$

From Fig. 3, the value under condition C is identical to that at A; simi-

TABLE III
RECALL FREQUENCIES IN ZEIGARNIK'S
EXPERIMENT

Tasks	Conditions of performance			
	A	B	C	D
1	9	8	—	—
2	—	12	9	—
3	—	7	7	—
4	—	12	12	—
5	4	—	—	14
6	—	10	5	—
7	5	—	—	8
8	—	13	8	—
9	10	—	—	9
10	—	16(17)	6(15)	—
11	6	—	—	8
12	—	5	2	—
13	—	14(17)	3(15)	—
14	—	7	3	—
15	6(15)	—	—	8(17)
16	—	5(15)	7(17)	—
17	7	—	—	11
18	—	8	2	—
19	7	—	—	13
20	—	7	6	—
21	—	15(17)	4(15)	—
22	—	9	8	—
Total	54 (127)	148 (242)	82 (222)	71 (113)
%	42.5	61.2	36.9	62.8

larly, the value at *D* is the same as that at *B*.

The foregoing analysis indicates that we should not expect any significant difference in recall between tasks preceded by completion and those preceded by interruption. On the other hand, interruption clearly enhances the probability of an interrupted task's recall by permitting additional stimuli to occur in contiguity with the task.

The results of Zeigarnik's original experiment are presented in Table III, compiled from information provided in the report (7, pp. 7, 8, and 10). Each cell entry represents the number of times a task was recalled after having

been performed under the specified condition. The possible maximum was 16, except in those cases where the maxima are shown in parentheses. A blank cell indicates that the task was never performed under the specified condition.

Because the four conditions are not represented by equal frequencies of performance, the recall frequencies are expressed as percentages of the possible maxima. A comparison of these percentages with the values computed above appears to lend credence to this analysis.

III. CONCLUSIONS

The foregoing discussion leads to several considerations, the most important of which is, perhaps, that of the experimental procedure. In the two experiments by Boguslavsky and Guthrie, contrasting results were obtained merely through variation in the sense modality of instructions. Other factors, such as the manner of interruption, the method of presenting a new task, time interval between the two, intervening activity, and a host of others, may well affect the results and deserve investigation.

Another consideration deals with the possibility of explaining individual differences in recall as variations in reactions to the experimental situation. Much of the current approach has consisted of the cataloguing of personality traits in relation to performance during recall. A more fruitful approach would be to relate performance during recall not to the behavior of the subject in some past clinical situation, but to his past reactions in situations similar to that of recall.

A final consideration is that the problem of the effects of interruption is essentially a problem in learning. As such, it fits within the framework of the existing theories of learning, rendering superfluous special explanatory devices, such as psychic tensions.

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WORD FREQUENCY, PERSONAL VALUES, AND VISUAL DURATION THRESHOLDS

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INTRODUCTION

If a psychologist is asked how a "perception" can be observed, he will usually reply that we really observe a verbal report or some other overt response taking place in the presence of certain stimulus patterns. Such an acknowledgment is what often passes for an "operational definition" of perception. Actually such apparent scientific sophistication has not always been significant within the psychology of perception. It is so easy to say that one *could* make such an operational definition if one really wanted to, that much of the literature on perception remains without specified operational definition of concepts. Perceptual *processes* like "seeing," "distorting," and "selecting," are concepts which are still used incorrectly in the psychology of perception. Such concepts often are private and imprecise, although presumably they need not be; and they often admit *ad hoc* explanations of certain observed relations between responses and stimuli. In the recent literature on perception, for example, we find statements such as the following: "A perception is an experience of something" (2, p. 14). "What one sees, what one observes, is inevitably what one selects from a near infinitude of potential percepts" (9, p. 142). "The goal of perception, in its broadest sense,

is the construction of a meaningful behavioral environment—an environment congruent with 'reality' on the one hand and the needs and dispositions of the organism on the other . . ." (2, p. 314). Such statements have been used as definitions of concepts in the theoretical formulation of certain experimental observations. Our feeling is that, if the operations that are basic to these concepts had been specified carefully, these statements would not have been made. They would have been recognized as redundancies, or at best, as assertions for which evidence is lacking in our present stage of knowledge.

We believe it is particularly important that the observed properties of those psychological events that are often described in such a quasi-subjective manner be specified with great care. For instance, if one takes the operational definition of concepts very seriously, it will be clear that a "perception" is defined by specifiable stimulus properties and specifiable response properties. One familiar type of experiment within the field of perception is characterized by the fact that human subjects *say* or *do* something in the presence of a situation established partly by optical operations and partly by verbal-instructional ones. Thus one is driven to specify the *response* properties that define perceptual concepts *as well as* the *situational* properties. It is all too conventional at present for perceptual behavior to be interpreted exclusively in terms of the situational properties—and, unhappily, for the situational properties to be defined in terms of private phenomenology.

¹ The second author is now at Tulane University.

² This research was supported by the Laboratory of Social Relations, Harvard University. The authors are indebted to Professor Leo Postman for many helpful suggestions in the preparation of this paper.

In this paper we are interested in the properties of a particular class of responses that can be labelled *linguistic*, since a predominance of recent perceptual data on human beings has been derived from the relationship between spoken reports and optical situations. Thus, the study of perception becomes in this case a study of the properties of linguistic responses relative to varying stimulus configurations. This paper is an attempt to show that so-called "perceptual phenomena" can sometimes be deduced directly from the relationship between certain known properties of the linguistic responses of individuals and certain properties of the optical and instructional situation in which those responses occurred. From the above analysis of the operations which define perceptual concepts it follows that in perceptual⁴ experiments in which linguistic responses are elicited from human subjects, *any variable that is a general property of linguistic responses must also be a property of any perceptual concept that is based upon those responses*. Here we shall consider one linguistic variable in particular: The *frequency of occurrence* of a word in a general sample of the English language. The perceptual concept based upon linguistic operations that we shall consider is *the duration for which the printed form of a word must be exposed in a tachistoscope, under a given set of instructions, before a subject will emit that word*. This is called the *visual duration threshold* of that word. We have treated the relation between these two variables elsewhere (4). In the present paper we wish to extend the analysis developed in that paper to a

somewhat more complicated topic in the field of perception.

To be more specific, let us examine a recent paper by Postman, Bruner, and McGinnies (9). These experimenters concluded that an individual's "interests" or "values" affect the duration thresholds for recognition of words that represent those values. They defined the duration threshold as the tachistoscopic exposure time required by a subject to recognize an exposed word. From the obtained relationship between values and visual duration thresholds these authors concluded that there were two perceptual processes in operation: *perceptual selectivity* and *perceptual defense*. Perceptual selectivity refers to the fact that in their experiment relatively *short* mean duration thresholds were found for words which represented those areas of interest that a subject valued highly. Perceptual defense refers to the fact that they found that words of low value areas had relatively *long* mean duration thresholds for correct report.

Postman, Bruner, and McGinnies went through the following experimental procedures. First, the Allport-Vernon Study of Values (1) was given to their subjects. This questionnaire investigates a subject's interests relative to six general areas: theoretical, economic, aesthetic, social, political, and religious. On the basis of scores obtained on this test, the orientation of each subject toward these six areas of interest was ranked from one to six.⁴

⁴ To clarify the terminology in this type of measurement, we shall use the word *interest* to designate the six fields of orientation; the word *value* will be reserved for the relative degree of orientation of a subject toward any one of these six areas of interest as indicated by his score on the Allport-Vernon questionnaire. Thus one can speak of the *value* a subject attributes to a particular area of *interest*, or one can speak of an individual whose two highest *values* lie in the theoretical and

³ Here the word *perceptual* does not refer to a process or a phenomenal or mentalistic event; it refers to a class of experiments that are usually included under the heading of perceptual experiments in various textbooks and reviews.

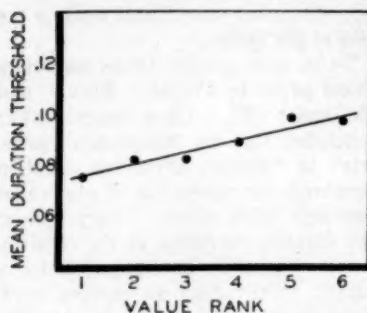


FIG. 1. Visual duration threshold (in seconds) as a function of Allport-Vernon value rank. These data are from Postman, Bruner, and McGinnies (6).

The experimenters then presented to their subjects 36 fairly common English words (six words representing each of the six interest areas), exposing these words in a tachistoscope and using an ascending method-of-limits procedure. The duration thresholds for each of the 36 words were obtained by determining the exposure duration which was just necessary to enable the subject to report correctly the word that was exposed in the tachistoscope. A slight tendency was found for words representing *highly valued* interests to have lower duration thresholds than words in *lowly valued* areas. A graph of this result is given in Fig. 1. To account for their findings the authors introduced two perceptual mechanisms. (1) "Value orientation acts as a sensitizer, lowering thresholds for acceptable stimulus objects" (e.g., words in a highly valued area) (9, p. 151). They named this mechanism selective sensitization. (2) "Value orientation may, on the other hand, raise thresholds for unacceptable stimulus objects" (9, p. 151). This mechanism

aesthetic areas of *interest*. The discussion which follows will center around *value rank*, which is the value a subject places upon a given area of interest relative to the values of the other areas of interest.

was called *perceptual defense*. Both of these hypothesized perceptual mechanisms must operate under subthreshold stimulation, since they obviously cannot be operative after the subject has met the threshold criterion by reporting a stimulus word correctly.

It must be emphasized that Postman, Bruner, and McGinnies derived their concepts from data consisting of *linguistic responses to linguistic stimuli* of a visual nature. Is it possible to account for their findings in terms of known properties of this relationship? We believe it is. It has long been known that the familiarity of an object is inversely related to the speed with which that object can be recognized. More specifically, we have shown in another paper (4) that the frequencies of occurrence of words in the Thorndike-Lorge word counts (11) show an inverse relation to the visual duration thresholds for those words. Sample data from this study are shown in Fig. 2.

Now let us assume that high valuation of a given area of interest is associated with a positive deviation from the mean frequencies with which words

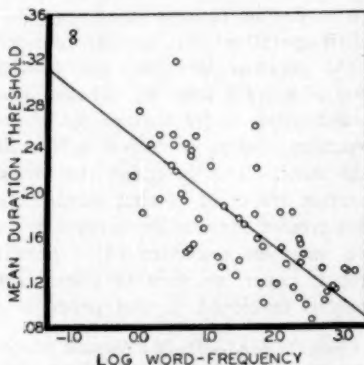


FIG. 2. Visual duration threshold (in seconds) as a function of Thorndike-Lorge word frequency. These data are from Howes and Solomon (3).

in that area occur in general usage. This assumption simply means that people who are highly interested in a subject generally use words associated with that subject more often than people who have no special interest in the subject. Positive and negative deviations in word-frequency thus would characterize the words that represent the extreme value ranks *one* and *six* of the Allport-Vernon test. The Allport-Vernon test is especially appropriate to this assumption, since it was validated on professional groups who were selected to represent the various categories of interest because they spent their full time in activities concerned with those interests.

This assumption, coupled with the experimental finding that an increase in the frequency of words is accompanied by a decrease in their duration thresholds, permits us to deduce the results of the experiment by Postman, Bruner, and McGinnies without having to postulate the two mechanisms of perceptual selectivity and perceptual defense. High value rank is correlated with a relatively high positive deviation from general word-usage—i.e., with relatively frequent use of a word. Frequency of use of a word is inversely related to the word's visual duration threshold, hence, the inverse relation between visual duration threshold and value rank.

This deduction of the Postman, Bruner, and McGinnies result has the advantage that it leads to certain other theorems that are susceptible to experimental investigation. From (a) the experimental relationship referred to above between visual duration thresholds for words and the Thorndike-Lorge word frequencies, and (b) the assumption that a given increase in the value placed upon a certain area of interest is correlated with a corresponding increase in word frequency for all words in that area, it follows that:

(1) The difference between the dura-

tion thresholds for words representing two areas of interest will be greater when there are relatively large differences between the actual value scores obtained for those areas of interest on the Allport-Vernon questionnaire. Thus the relation between duration threshold and value *rank* will depend upon the test scores from which the value ranks are derived. Subjects who show extreme *scores* on the Allport-Vernon test should yield larger differences between the mean duration thresholds for value *ranks* one and six, for example, than subjects who show only small variations in their value scores.

Since the relationship between visual duration threshold and word frequency approximates an inverse logarithmic function (see 4), a given change in word frequency will produce a smaller change in duration threshold if it occurs for a word of relatively *high* frequency than if it occurs for a word of relatively *low* frequency. This consideration leads to a second deduction:

(2) A given difference between two value ranks will produce a smaller difference in duration thresholds for relatively *frequent* words representing the two areas of interest than in the thresholds for relatively *infrequent* words representing those interests. A quantitative treatment of this deduction is presented in Appendix 1.

The fact that in our experiments we were able to obtain stable relationships between visual duration threshold and word frequency with less than 20 subjects is strong evidence that the frequency of occurrence of a word varies only slightly from person to person in comparison to extreme variations of frequency of occurrence from word to word. This conclusion also has a strong basis in everyday observation of language behavior. If we accept this inference, we can state a third deduction:

(3) Differences between the duration

thresholds of words representing extreme differences in value rank will be small compared with differences between the thresholds of words representing extreme differences in word frequency in the Thorndike-Lorge word counts.

Now let us turn to the experimental test of these three deductions.

PROCEDURE

This experiment is essentially a repetition of the experiment reported by Postman, Bruner, and McGinnies (9). Their stimulus words, however, were uncontrolled for frequency of occurrence in the Thorndike-Lorge word counts. Consequently we devised a new list of words representing the six areas of interest. To be certain that the words we used could be called representative of the fields of interest to which they were assigned, we selected words that were used to represent the respective interests in the actual questions on the Allport-Vernon test. Five words were so chosen for each of the six interest areas. These 30 words constitute the *relatively frequent* words of

our experiment, and they are listed in Table I. Each of the 30 *relatively infrequent* words shown in Table I was chosen to match one of the 30 frequent ones with respect to interest category, and to differ from it in having a considerably *lower* frequency of occurrence in the Thorndike-Lorge counts. The procedure was to select a relatively infrequent word from the lists of cognate words found in a standard thesaurus (8). Synonyms were chosen wherever possible. Care was taken that each relatively infrequent word did not differ greatly in length from its relatively frequent cognate. No words less than six letters long were selected.

At each experimental session the subject was first given the Allport-Vernon Study of Values. Then the 60 words listed in Table I were presented in the tachistoscopic procedure. This procedure has been described in detail in a previous paper (4). In brief, each word was first exposed for a duration too short to permit the subject to report it correctly. The duration of succeeding exposures was then increased gradually until the subject reported the

TABLE I
STIMULUS WORDS

	Theoretical	Economic	Political	Eathetic	Religious	Social
Relatively frequent	scientific physics intellectual knowledge education	automobile market savings economics earthly	liberties lawyer debating initiative government	poetry picture painter orchestra literary	churches heavenly spiritual reverence religious	hospitality friendly service charitable sympathy

(Mean log frequency = 2.23)

Relatively infrequent	inductive statics percipience erudition pedagogue	limousine broker frugality assets mundane	uncoerced barrister rebuttal assiduous judiciary	elegies vignette etcher ensemble metaphor	chancels celestial psychical beatific theistic	conviviality amicable benign altruistic condolence
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(Mean log frequency = 0.60)

TABLE II

MEAN DURATION THRESHOLDS (IN SECONDS) FOR FREQUENT AND INFREQUENT WORDS
STIMULI OF EACH VALUE RANK

	Value ranks						Means
	(High) 1	2	3	4	5	(Low) 6	
Frequent words	.132	.126	.127	.124	.146	.123	.129
Infrequent words	.211	.212	.198	.227	.197	.231	.213
Means	.172	.169	.163	.176	.172	.177	

word correctly. A duration threshold for each word was obtained separately in this way for each subject. Nineteen subjects were used.

RESULTS

The value ranks for each subject were derived from his scores obtained on the six areas of interest in the Allport-Vernon test. The highest value area for a given subject was given a rank of one and the lowest a rank of six. The visual duration thresholds for words representing each value rank were then averaged over the group of 19 subjects, regardless of what specific field of interest happened to receive that value rank. For example, if Subject A scored highest on the Allport-Vernon test in the area of theoretical interest and Subject B scored highest in the aesthetic area of interest, then Subject A's duration thresholds for the theoretical words (scientific, physics, intellectual, knowledge, education) would be averaged with Subject B's thresholds for the aesthetic words (poetry, picture, painter, orchestra, literary) in order to obtain the mean duration threshold for frequent words in *value rank one*. Thresholds for frequent and for infrequent words were averaged separately. Thus the mean duration threshold for each value rank, either for frequent or for infrequent words, is based upon a total of 95 threshold measurements. These

means, along with the means for frequent and infrequent words combined ($n = 190$) are shown in Table II.

Inspection of these data shows no indication of systematic variation of visual duration threshold with value rank. There is, however, an appreciable difference between the mean thresholds for frequent and infrequent words at every value rank.⁵ The results shown in Table II thus substantiate our third prediction, that threshold differences associated with differences in value rank will be small compared with those associated with differences in word frequency. It is clear that the data in

⁵ Two controls show that these differences between the mean duration thresholds for frequent and infrequent words were not exaggerated by the appearance of frequent words in the Allport-Vernon test before their tachistoscopic presentation: (1) A controlled experiment using words selected randomly from the Thorndike-Lorge word lists yielded the same function relating visual duration threshold and word frequency as did the 60 words of this experiment (see 4); (2) one half of the subjects used in the present experiment received the Allport-Vernon test two weeks prior to the tachistoscopic part of the experiment, while the other half received the tachistoscopic test immediately after filling in the Allport-Vernon questionnaire. These two groups of subjects showed the same differences in duration thresholds for frequent and infrequent words. It might also be mentioned that none of the subjects reported that he was aware that any of the words appearing in the tachistoscopic experiment had appeared in a question on the Allport-Vernon test.

TABLE III

MEAN DURATION THRESHOLDS (IN SECONDS) FOR FREQUENT AND INFREQUENT WORD STIMULI IN EACH VALUE RANK FOR A SELECTED SAMPLE OF ELEVEN SUBJECTS

	Value rank						Means
	(High) 1	2	3	4	5	(Low) 6	
Frequent words	.114	.105	.119	.112	.131	.125	.118
Infrequent words	.195	.206	.182	.211	.204	.221	.203
Means	.155	.156	.152	.162	.168	.173	

Table II do not bear out those reported by Postman, Bruner, and McGinnies which were plotted in Fig. 1. However, what indication there is of a general tendency in the data seems to point in the direction of lower thresholds for the high value ranks: The mean duration threshold for the highest three value ranks is slightly lower than the mean duration threshold for the lowest three ranks both for the frequent and the infrequent words. The mean duration thresholds for value ranks *one, two, and three* combined, for the frequent and the infrequent words, are 0.128 and 0.207 second, respectively. For value ranks *four, five, and six* combined, the corresponding means are slightly higher, 0.131 and 0.218 second, respectively.

Now if our first deduction is substantiated, it should be possible to enlarge the threshold differences from value rank to value rank by selecting the data for those subjects who show extreme scores on the Allport-Vernon test. We therefore set aside for separate treatment those eleven subjects whose Allport-Vernon scores exhibited unusually large departures in any interest area from the standardized norms for that test (see 1). The duration thresholds for these eleven selected subjects are given in Table III and are plotted against value rank in Fig. 3. An examination of Fig. 3 shows that the trend for the eleven selected subjects is obviously in the direction found by Postman, Bruner, and McGinnies. When

TABLE IV

ANALYSIS OF VARIANCE OF DATA IN TABLE III

Source	Sum of squares	df.	Mean square	F	P
Total	129,728.39	654	—	—	—
S's	38,278.77	10	3,827.88	32.53	*
F	12,278.77	1	12,883.26	109.49	*
V's	1,138.29	5	227.66	1.93	—
S × F	6,208.92	10	620.92	5.28	*
S × V	2,845.61	50	56.91	.48	—
F × V	946.21	5	189.24	1.61	—
Error	67,427.33	573	117.67	—	—

* Significant at 1 per cent level.

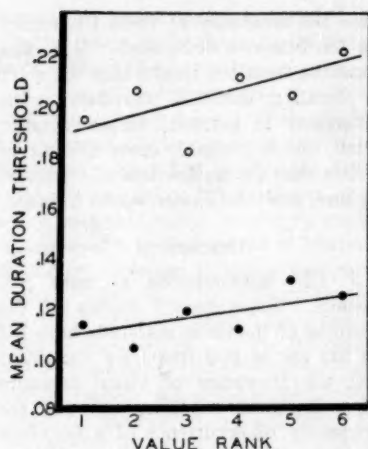


FIG. 3. Visual duration threshold (in seconds) as a function of Allport-Vernon value rank with word frequency as a parameter. The upper curve (hollow circles) is for relatively infrequent words (mean log word frequency = 0.60). The lower curve (black dots) is for relatively frequent words (mean log word frequency = 2.23). The straight lines have been fitted by the method of least squares. They serve merely as convenient indications of central tendencies of the data, and should not be interpreted to signify a rectilinear function between duration threshold and value rank, since value rank is not a continuous dimension and the size of intervals between ranks is unspecified.

the three highest and three lowest value ranks are compared, the differences between mean duration thresholds for high and low value ranks are greater than those observed for all 19 subjects. The mean duration thresholds for value ranks *one, two, and three* combined, for the frequent and the infrequent words, are 0.113 and 0.194 second, respectively. For value ranks *four, five, and six* combined, the corresponding means are 0.123 and 0.212 second, respectively. The fact that the relation between duration threshold and value rank is clearer with subjects selected for extreme value scores is in agreement with the first deduction from the as-

sumptions discussed in the introduction.

The data on the 11 selected subjects are also congruent with our second deduction, since the infrequent words exhibit considerably greater variation in mean duration threshold as a function of value rank than do the frequent words. In addition, the third deduction receives strong support from these data, since they show that even when subjects are selected for their extreme variations in Allport-Vernon score the differences in duration thresholds associated with differences in value rank are quite small compared with the difference between duration thresholds for frequent and infrequent words. Thus all three deductions correspond with the actual tendencies observed in our experimental data.

An analysis of variance for the data of the eleven selected subjects is summarized in Table IV. By far the largest estimate of variance is that associated with differences in word frequency or familiarity. Individual differences among subjects also yielded a large estimate of variance. The F-ratios for both of these sources of variance are significant at the 0.01 level.⁶ The differences among duration thresholds for words in the different value ranks are not significant according to the F-test. However, as we have pointed out, there is a marked trend for high-value words to have shorter duration thresholds. The difference between mean duration thresholds for infrequent words of value ranks one and six is significant at the 0.05 level.

The extremely low variance attributable to the interaction between subjects and value ranks indicates that the subjects behaved quite uniformly with respect to the way their thresholds vary

⁶ This is true whether the F-ratios are based upon the error term or upon the significant interaction variance attributable to subject \times word frequency.

with value rank. The interaction between frequency and value rank, although it is not statistically significant, is large in comparison with the variance contributed by value rank alone. This result may indicate that the difference between the two slopes in Fig. 3 might well be statistically significant in an experiment in which value rank emerged as a variable significantly associated with duration thresholds.

Of our three deductions, only the third—concerning the relative importance of word frequency and value rank as variables related to variation in duration thresholds—is adequately tested by our statistical analysis. Its confirmation is striking. The present data do not permit an adequate statistical test of the first deduction: that value ranks based upon the extreme scores in the Allport-Vernon test will be associated with greater differences in duration thresholds than will value ranks based upon the relatively small differences in test scores. This deduction, however, is supported by a comparison of the data in Tables II and III. The second deduction, which requires that the slope of the relationship between duration thresholds and value ranks should vary with the Thorndike-Lorge frequencies of the two groups of words used in this experiment, cannot be tested legitimately by an analysis of variance for these data. This follows from the fact that we found no significant variation in value duration threshold to be associated with variation in value rank. The data of Table III, however, are certainly consistent with the deduction, and an examination of the quantitative data presented in Appendix I lends additional support to it.

Thus, in summary, the three deductions from our general assumptions describe the outstanding trends in the data, but additional experiments will certainly be necessary in order to deter-

mine the reliability of these tendencies for the first two deductions. It is also apparent from our results that the shift in duration threshold correlated with differences in personal values is quite small, and is probably more difficult to obtain than the earlier data of Postman, Bruner, and McGinnies would indicate.

DISCUSSION

1. *The interpretation of word frequency.* Experimental studies of the learning of linguistic materials have led to the use of two frequency variables: (a) the frequency of visual exposure of a linguistic stimulus, and (b) the frequency of occurrence of a linguistic response. It is theoretically possible to distinguish between these two frequency variables in any experiment utilizing linguistic materials, but a word count such as the Thorndike-Lorge list does not do this. For the sake of simplicity, we have in this paper considered Thorndike-Lorge word frequencies as estimates of the frequency of words "in a general sample of the English language." This is, of course, an inexact formulation. The frequency of a word in such a list can be interpreted either as a stimulus or as a response variable. In the first case a Thorndike-Lorge frequency is considered as an estimate of the frequency with which a word is exposed during the reading of a population of subjects who read the class of printed material that is analyzed in the word count. In the second case the word-count frequency is considered as an estimate of the frequency with which a word is emitted by the population of subjects who wrote the material sampled by the count. It seems reasonable to expect that there would be a high correlation between these two frequency variables, but whether visual duration threshold is related to each of them in the same way remains to be seen. In the absence of further data it seems best

merely to define word frequency for English words as the frequency of words in the Thorndike-Lorge tables.

2. *The Allport-Vernon test as a corollary of word frequency.* "Value," "interest," and similar concepts, we have suggested, are often inferences from word frequencies. If a subject uses many economic words relative to words representative of other areas of interest, he is often classified as having economic "values." Scores on the Allport-Vernon test must somehow reflect such differences in word usage. In the introduction we considered this relation between Allport-Vernon score and word frequency as a separate assumption. In this section we wish to show that it can be stated as a corollary of differences in word frequency and the experimentally demonstrated function between duration threshold and word frequency. If this can be accomplished, only one principle will be necessary for the description of our own results and those of Bruner, Postman, and McGinnies.

Let us examine carefully the operations that determine what score a subject will receive when he is given the Allport-Vernon test. The Allport-Vernon test can be considered as a set of visual choice discriminations between alternative groups of words, the choice being presented as a series of paired comparisons. Each comparison is between words representing two different interest areas. For example, consider item 4 of the test: "If you were a university professor and had the necessary ability, would you prefer to teach: (a) poetry; (b) chemistry and physics?" If, in a series of items like this, a subject always chooses the poetry side of the question, we say that he has strong aesthetic values, and we assign a value rank of one to the aesthetic area for that subject. Value rank, therefore, reflects the frequency with which words

representing one interest area are chosen in preference to words representing another interest area.

Now let us consider two populations of subjects, A and B, which are identical except for the fact that A uses the word *poetry* more frequently than B does, and that B uses *chemistry* and *physics* more often than A does. What will happen when they are presented with a questionnaire item such as item 4 from the Allport-Vernon test? We know from other data (4) that the duration threshold of *poetry* will be lower for A than for B, and that the duration thresholds for *chemistry* and *physics* will be lower for B than for A. Let us then imagine a questionnaire item in which the subject responds *vocally* to the printed form of the two possible choices instead of checking a number that corresponds to the printed form of the choice word (e.g., the numeral 1 to *poetry*). Since on this test the operations performed are identical to those defining duration threshold, we know from (4) that, following any given amount of exposure to the printed alternatives, the word with the higher frequency of use will tend to occur more often. For our two populations A and B, therefore, there will be a tendency for A to make the response "poetry" more often than B, and a tendency for B to make the response "chemistry and physics" more often than A. On a modified Allport-Vernon test composed of such items, therefore, population A will be said to have higher values in aesthetics than B, and B will be said to have higher values than A in the theoretical area, *although it has been assumed that the only difference between the two populations is the frequency with which they use the two sets of words.*

To generalize this argument from our hypothetical questionnaire to the actual Allport-Vernon test involves one major

assumption, *viz.*, that the response of checking a number that corresponds to a printed word does not differ fundamentally from the response of emitting the vocal sounds; that it corresponds to the frequency with which that word has been emitted in the past as a "reading response" in the presence of the visual exposure of its written form. To make this interpretation it is necessary to postulate that covert ("implicit") word-responses, which cannot actually be observed, occur during reading. On the other hand, it permits value rank and duration threshold to be viewed as two derivatives of the strength of association between linguistic stimuli and covert linguistic responses. In that way it provides a unified description of the results presented above.

According to association theories, the more frequently a response has been associated in the past with a stimulus, the briefer the exposure that is necessary to elicit that response overtly. Now it is also possible to consider the Allport-Vernon questionnaire in the association paradigm. The Allport-Vernon scale can be considered as a set of visual choice discriminations between alternative groups of words, the choice being presented as a series of paired comparisons. For example, consider again item 4 of the test: "If you were a university professor and had the necessary ability, would you prefer to teach (a) poetry; (b) chemistry and physics?" The printed words *poetry* and *chemistry or physics* are the discriminative stimuli and the circling of an answer is the overt response in this discrimination. In conventional association interpretation this response is assumed to be mediated by the covert response of one of the alternative word-pairs, and the appropriate circling response will depend upon which mediating response is more strongly associated with its visual stimulation. According

to all association theories of verbal learning the covert word-response that has been more often associated with its discrimination stimulus will tend to be chosen more often. We can as yet advance no experimental data for this assumption. Nevertheless both types of response are learned as appropriate responses to printed words, so that the association is of the same derivation in both cases. If we are willing to accept the assumption for the time being, we can deduce a subject's score on the Allport-Vernon test from the inverse relation between word frequency and duration threshold, plus a knowledge of how frequently that subject uses words representing the various areas of interest.

3. *Antecedent conditions related to word frequency.* Word frequency has been treated throughout as a property of the state of the organism at the time of measurement of duration thresholds. The interrelations of word frequency, duration threshold, and personal values are thus cross-sectional with respect to time. Frequency of occurrence in the Thorndike-Lorge counts, however, is not in itself an independent variable that is capable of experimental manipulation. Our analysis of the approximately simultaneous determinants of duration threshold, therefore, needs to be supplemented by methods of manipulating word frequency as it has been defined in the above experiments. Two general approaches may be suggested: (a) We can artificially establish differential word frequencies in a laboratory situation, observing their effects upon subsequent perceptual phenomena; (b) we can specify particular antecedent conditions (independent variables) that are sufficient to establish word frequencies as a variable dependent upon those conditions. When, after building word frequencies into the organism by these procedures, we can accurately predict related perceptual phenomena, we may

find that many so-called "determinants" of perception will be by-passed or will become redundant.

The first approach may be illustrated by a preliminary experiment (10). Word frequencies were produced artificially by having Turkish words—completely unfamiliar to English-speaking subjects—pronounced different numbers of times. Word frequency was in this way established as an independent variable capable of direct manipulation. Measurement of duration thresholds of those words followed, and the relation between the threshold for a word and its frequency of previous pronunciation was found to approximate the relation between duration thresholds for English words and Thorndike-Lorge word frequencies.

The second approach to the manipulation of word frequency depends upon its being related to a system of genetic or historical relationships, such as those basic to current learning theories. Among the best established propositions in modern association theory are those that relate the *frequency of occurrence of a learned response* (as a dependent variable) to specified conditions under which the response was learned (independent variables). These independent variables are extremely varied, including such factors as reinforcement, rate of repetition, time between repetitions, etc. A special case of this dependent variable of learning experiments is the frequency of occurrence of words, since words are learned responses. All of the independent variables above, consequently, should permit the control of word frequency and indirectly of duration thresholds for words if the propositions of learning theory concerning response probability are valid. When viewed from this perspective, the relation between word frequency and duration threshold is a case of the general relationship between the probability of

a response and the latency of its occurrence in the presence of a discriminated stimulus. In this respect it bears upon a fundamental problem in learning theory, since response probability and response latency are sometimes considered as two manifestations of the single, basic concept of habit strength.

McGinnies (7) and others (6) have argued that "emotional" determinants operate selectively in the tachistoscopic situation in determining visual duration thresholds. We do not believe there is sufficient experimental evidence to justify such a conclusion. At present one can, with reasonable precision, account for the duration thresholds of words classified as emotional, as well as of neutral words, simply in terms of their Thorndike-Lorge frequencies, without making additional assumptions (5). The discussion of the preceding paragraph should make it clear that this does not mean that we consider "emotional" determinants unimportant to the analysis of language behavior. Emotional factors undoubtedly operate to an important extent in the *building* of word frequencies in a given life history. In this way they would be related to word frequency and, indirectly, to duration threshold in the manner suggested above for other antecedent conditions of learning. But to date we can find no evidence to suggest that emotional factors operate in the tachistoscopic situation independently of their effect upon word frequency.

The investigation of duration thresholds for words has only begun. We are well aware that the frequency of use of a word is not the only property of that word that is related to its duration threshold. Nor do we wish to over-emphasize the response attributes of perceptual data. Stimulus generalization, for instance, is also evident in tachistoscopic experiments such as the one we have described. Thus when the

word *surmise* is exposed very briefly in the tachistoscope, the word *surprise* is often the response that is given; the frequency with which such a generalized response occurs, moreover, appears to depend upon the Thorndike-Lorge frequency of that word, among other things. Many such properties of the events occurring in such experiments need to be investigated before an adequate theoretical account of the data can be given. Nonetheless, the relationships we have reported previously (4, 5), taken together with those presented in this paper, strongly suggest that the Thorndike-Lorge frequency of a word will probably turn out to be an important variable in any such theory. And the preceding discussion demonstrates how inquiring more deeply into the nature of the observations that determine certain customary psychological concepts may lead to a more coherent organization of the body of knowledge that concerns those concepts. From this point of view, it is reasonable to expect that the theoretical development of much of the psychology of human perception will be interrelated closely with the development of the psychology of language behavior.

APPENDIX I

Quantitative statement can be given to the second deduction, that the function relating duration threshold to value rank will vary with the Thorndike-Lorge frequency of the words representing the various fields of interest. First of all, let us restate the basic assumption made in the introduction to this paper, that, for words representing a field of interest, valuation of that interest is associated with a departure from the mean frequency of use in the general population. If p is the probability (relative frequency) in the general population of any word in a given field of interest, a high value placed on that

field by a subject thus will mean that the probability of such a word for that subject will be greater than p by some quantity Δp . In terms of the operations employed in our experiment, a high score in theoretical interest on the Allport-Vernon test would mean that, on the average, the probabilities of words representing theoretical interest (e.g., *scientific* and *inductive*) will be increased over their Thorndike-Lorge values by some quantity Δp .

The second basic assumption concerns the relation between duration threshold and word frequency. This relation can be written (4)

$$t = -k \log f - C, \quad (1)$$

in which t is the mean duration threshold (in units of time), f the Thorndike-Lorge frequency of the word, and k and C are constants. Considerable evidence indicates that this relationship holds over the entire range of word frequencies represented in the present experiment (3, 4). These data also show that the constant C has the value zero when a constant, 4.4, is subtracted from $\log f$. Since this difference amounts to the division of a set of frequencies by a constant that is larger than any member of that set of frequencies, it assumes certain properties of a relative frequency or probability. Let p stand for the probability of a word based on such a relative frequency. Then

$$\log p = \log f - 4.4 \quad (2)$$

and

$$t = -k \log p. \quad (3)$$

Now let us take two words with probabilities p_1 and p_2 that represent the same area of interest. Our assumptions permit us to describe the probabilities of those words for an individual who has positive value in that field of interest as $(p_1 + \Delta p)$ and $(p_2 + \Delta p)$, respectively. The corresponding duration

thresholds are given by

$$t_1 + \Delta t_1 = -k \log (p_1 - \Delta p), \quad (4)$$

$$t_2 + \Delta t_2 = -k \log (p_2 - \Delta p), \quad (5)$$

in which t_1 and t_2 are the thresholds corresponding to p_1 and p_2 , and Δt_1 and Δt_2 are the changes in threshold introduced by the addition of Δp to p_1 and p_2 . Assigning the value zero to t_1 and t_2 to simplify comparison, we have

$$\Delta t_1 = -k \log (p_1 + \Delta p), \quad (6)$$

$$\Delta t_2 = -k \log (p_2 + \Delta p). \quad (7)$$

When Δp is small,

$$\Delta t_1 / \Delta t_2 = \log p_1 / \log p_2. \quad (8)$$

To express this result in words, our assumptions imply this equation: The differences in duration thresholds for *infrequent* and for *frequent* words introduced by a given difference in value rank will stand in the same proportion to each other as the ratio of the logarithms of the Thorndike-Lorge probabilities of the two sets of words.

Mean logarithms of word frequencies for both frequent and infrequent words

are shown in Table I. By Equation 2 the mean probability for the 30 infrequent words is $0.60 - 4.40 = -3.80$; that for the 30 frequent words is $2.23 - 4.40 = -2.17$. The ratio of these values of $\log p$ is $1.75 +$. The difference in duration threshold associated with differences in value rank, Δt , can best be computed from our data as the difference between the mean duration threshold for the highest three value ranks averaged together and that for the lowest three value ranks averaged together ($N = 165$ for each mean). Table V compares the ratio of log word probabilities with the corresponding ratio of differences in duration threshold. For the data on 11 subjects with extreme scores on the Allport-Vernon test, these ratios are almost identical. The threshold differences for the total group of 19 subjects are so small that they are less than the smallest unit of measurement to which duration thresholds were determined (0.01 sec.), but yield a ratio that is of the order of magnitude to be expected from the ratio of word probabilities.

Although these data are suggestive, they are of course insufficient to establish the particular set of assumptions we have employed. It is to be hoped

TABLE V

RATIOS OF DIFFERENCES IN DURATION THRESHOLDS FOR FREQUENT AND INFREQUENT WORDS COMPARED WITH THE RATIO OF MEAN LOG WORD FREQUENCIES

Type of measurement	Duration thresholds		Ratio: Infrequent/Frequent
	Infrequent	Frequent	
1. Ratios of log word probabilities	—	—	1.75+
2. Lowest 3 value ranks minus highest 3 (11 subjects)	0.018	0.010	1.8
3. Lowest 3 value ranks minus highest 3 (19 subjects)	0.011	0.003	3.7
4. Value rank 6 minus value rank 1 (least squares fit of Fig. 3)	0.027	0.015	1.8

that comparable data from other laboratories will soon permit a more extensive test of the deductions. Even at the present stage, however, we believe this formulation illustrates the potentiality of a concrete statement of the operations involved in the measurement of "perception." Such a statement makes it possible for a single unambiguous formulation to describe complex relationships among perceptual data which quasi-subjective formulations like perceptual defense and perceptual sensitization could not reveal.

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PERSONAL VALUES, VISUAL RECOGNITION, AND RECALL¹

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There is considerable evidence that past experience and motivation may under certain conditions significantly influence perceptual responses. An individual may give different perceptual responses to the same physical stimulus on different occasions, and individuals may differ from each other in their responses to the same situation. Not all of such variability need be ascribed to "error" variance; rather, it can be systematically related to "directive" factors in the subjects—their drive states, lasting predispositions and momentary sets, past experiences and expectations.² The pragmatic value of such variables cannot be seriously questioned if their use increases precision of analysis and prediction in the study of perception.

Let us assume for the moment that the effectiveness of directive factors in perception has been successfully demonstrated. What are the theoretical implications of such findings? One view would be that in the light of such results perceptual organization should be considered as *jointly determined* by sensory and directive factors (2, 3, 9).³ Even-

tually both sets of factors may be translated into a common language (5), but for experimental purposes it is necessary to distinguish them and to vary them independently. Those who hold this view will be partial to theoretical constructs defined in terms of both sensory and directive variables, so that the subject's perceptual behavior may be linked to both types of factors. The same type of approach can be extended to other processes such as memory (9) in the hope of establishing general principles of cognition.

Alternative interpretations of the experimental facts are possible. Motivational factors and past experience may affect *judgment or motor (e.g., verbal) response but not perception*. This view is held, for example, by Pratt (12) who argues that past experience may affect the subject's overt (motor) response but that its effect on sensory and perceptual events is negligible. Whenever past experience does seem to affect perception, we are probably dealing with the modification of a motor response. Pratt concludes that "sensory and perceptual dimensions are stimulus-bound. The exceptions are so uncommon that when they seem to appear they should be scrutinized critically, if not incredulously" (12, p. 106). We are then faced with a serious problem of analysis. Given variations in responses of sub-

Crutchfield (6) between structural and functional factors.

¹ This research was facilitated by a grant from the Laboratory of Social Relations, Harvard University. We are greatly indebted to Dr. Richard L. Solomon for many stimulating suggestions and criticisms.

² Surveys of recent theoretical and experimental developments in this area may be found in *Perception and personality: a symposium*, Duke University Press, 1950.

³ The distinction between sensory and directive factors parallels that made by Krech and

jects who differ in past experience and motivation, we have to disentangle the truly perceptual (stimulus-bound) components of their behavior and those which are due to response modification. Since the operations for the observation of perceptual changes and changes in judgment (motor responses) overlap, a clear separation of perceptual and response components may not always be possible.

The Gordian knot can be cut in still another way. Recognizing that perceptions and judgments are never directly observed but rather inferred from variations in motor and/or verbal responses, one may ask why the concepts of perception and judgment need to be retained at all. Is it not preferable to relate variations in physical stimuli and directive factors to changes in motor and verbal responses and to dispense with the concept of perception altogether? Lawful relationships between stimulus conditions and responses can be worked out directly without recourse to intervening "mentalistic" constructs such as perception. Such a view is championed by Solomon and Howes (13). They argue that the study of perception can be reduced to "a study of the properties of linguistic responses in the presence of varying stimulus configurations" (13, p. 257). In short, the study of perception is to be absorbed into a scheme of stimulus-response analysis.

One important implication of such an approach is the need for careful analysis of the properties of the subject's responses in any given perceptual situation. Performance variables as well as stimulus variables must be fully specified. Again in the words of Solomon and Howes, "any variable that is a general property of linguistic responses must also be a property of any perceptual concept that is based upon those responses" (13, p. 257). Take, for ex-

ample, word stimuli presented for recognition in a tachistoscope. Perceptual recognition of the stimulus is inferred when the subject pronounces (or writes down) the stimulus word. Some verbal responses, however, have a higher probability of occurrence than others due to differences in frequency of past use (or, perhaps more accurately, more frequent associations between seeing and pronouncing the word). Such differentials in response strength lead to differences in speed of recognition (4, 13) and hence to differences in inferred perceptual sensitivity. It is only through analysis of the properties of the responses as well as the properties of the stimulus that such variations in perceptual recognition can be fully understood. In terms of such an analysis, directive factors cease to be a special problem. Clearly the probability of a given response occurring in the presence of a given stimulus depends to some extent on the motivational state of the individual, the frequency with which the response has occurred in the past, the consequences of the response, etc. The scheme of analysis that emerges is that of S-R learning theory.

It is true that, in strict operational terms, the study of perception is the study of verbal and/or motor responses in the presence of varying stimulus configurations. It is, indeed, possible to describe many, if not most, experiments in human behavior in these terms. Recognition of this fact does not necessarily deny the usefulness of distinguishing among various classes of events, including perhaps perception! It may still be valuable to search for laws which are not general stimulus-response laws but hold only for a limited class of events. Can we, for example, maintain a reasonable distinction between perceptual responses and other types of responses? We are inclined to believe so. The history of perception is long and rich, and

most of the time investigators have seemed to be in substantial agreement as to what constituted their field of study.

Let us, therefore, make explicit what researchers on perception have in fact been doing. Those variations in response can be designated as *perceptual* that satisfy the following criteria: (1) stimulus conditions and/or directive factors are systematically manipulated by the experimenter; (2) the subject is explicitly instructed or "set" to describe, or to make discriminatory responses to, objects that are present in the environment. The setting of the organism to respond to objects that are actually present in the environment is an important part of the operations defining perceptual events. Writing of the operational definition of perceptual attributes, Stevens justly argued that "this tuning of the observer is one of the fundamental operations underlying the concept of attribute. The ability of the experimenter to set the observer, for example, to respond to loudness and not to pitch is crucial to the determination of the attribute *loudness*" (14, p. 525). By the same token, it is the setting of the subject to respond to objects and events in the environment along specific dimensions of discrimination which is the hallmark of perceptual responses in general. Whether or not this particular formulation is acceptable, it should be possible to agree on a set of operations—manipulations of stimuli and of subjects—which define the area of perception. Variations in response resulting from these operations are then treated as perceptual responses, and the laws pertaining to this particular class of responses are the laws of perception. Some of these laws may, indeed, hold for other classes of responses as well while others are specific to perception.

It is the very fact that some laws hold equally well for perceptual responses and other classes of responses

which makes it possible to put the perceptual processes in a broader behavioral context. We can, for example, specify a set of operations which will constitute the area of *cognition*. This broader set will include the operations defining perception as well as those defining remembering and thinking. The question can then be raised whether there are general laws or principles of cognitive response which are true for all the specific cognitive functions. If the answer is yes, we can progress in the development of a theory which embodies the general principles cutting across specific cognitive functions.

In developing such a theoretical scheme we shall do well not to prejudge what factors will produce significant variations in a given class of responses. Whether so-called directive factors can affect perceptual responses, as we have defined them, is an empirical question. It is a question, moreover, that is not likely to have an all-or-none answer. The information that is available to date points to the conclusion that directive factors do have demonstrable effects over a limited, specifiable range of conditions.

Let us summarize the preceding discussion. There is empirical evidence, highly suggestive if not firmly established, that directive factors are significant determinants of perceptual behavior. At the present state of our knowledge it is premature to relegate such facts to "mere" phenomena of judgment and motor response while keeping "pure" perception stimulus-bound. Nor are we willing to solve the theoretical problem by absorbing the study of perception into a general scheme of stimulus-response analysis in which the concepts of perception and perceptual organization disappear, with directive factors and stimulus factors treated as conditions of *response probability*. We shall, instead, continue to

infer changes in perceptual organization from systematic variations in response produced by a circumscribed set of operations—manipulation of physical stimuli and directive factors, including instructions to the subject to report on objects immediately present in the environment. Some laws established in such experiments may be peculiar to perception; others are more general laws of response that hold true for a larger range of events. For a full understanding of the behavior in a perceptual situation, the investigator will, therefore, often have to go beyond specifically perceptual laws and constructs. He will have to inquire what portion of the observed variance in perceptual response can be accounted for in terms of laws of response not peculiar to perceptual responses but true for a variety of other situations as well. In this way we shall remain mindful of the continuity of perception and other forms of behavior.

PERSONAL VALUES AS DETERMINANTS OF COGNITIVE BEHAVIOR

These theoretical considerations will now be applied to a specific case: the effects of personal values on perceptual sensitivity to stimuli related to these values. The history of this problem illustrates well the necessity to analyze the observed variance in perceptual behavior both in terms of peculiarly perceptual variables and more general laws of response. The errors of omission in early investigations of this problem are proving to be very instructive.

The relationship between personal values and perceptual sensitivity to words was first explored by Postman, Bruner, and McGinnies (10). The Allport-Vernon Study of Values was used to measure the major values of the subjects.⁴ The speed with which

subjects recognized words (presented tachistoscopically) relevant to the six value areas of the test was related to the subjects' value profiles. A significant relationship was found: The higher the rank of the value, *i.e.*, the more prominent it was on the value profile, the more rapid was the average speed of recognition of words relevant to that value.⁵ In interpreting these results, such perceptual mechanisms as "selective sensitization" and "perceptual defense" were invoked. Selective sensitization was used to explain the lowering of thresholds for high value words; perceptual defense referred to a mechanism by which thresholds for low value words were raised. These constructs were not entirely *ad hoc* and were supported by a number of other experimental findings (2).

As it now turns out, an important factor was omitted from consideration in the analysis of these results. Howes and Solomon (4) have since shown that there is a high correlation between the frequency with which words occur in the English language and the speed with which they are recognized in the tachistoscope. The relationship between duration thresholds and frequency of usage as measured by the Thorndike-Lorge word counts is well described by a linear logarithmic function. This factor of relative familiarity of the stimuli was not controlled in the study of Postman, Bruner, and McGinnies nor was this variable partialled out in the analysis of the results. The question at once arises whether the effect of personal values on perceptual sensitivity to words can be fully accounted for in

areas: theoretical, economic, political, esthetic, religious, social. On the basis of the relative scores for these values, the subject's "value profile" is plotted.

⁵ Similar findings were reported by Vanderplas and Blake (16) who used auditory rather than visual presentation of value-relevant words.

⁴ The Allport-Vernon Study of Values (1) measures the relative strength of six value

terms of the relative familiarity of the stimuli. Can we substitute a general principle of response probability for such peculiarly perceptual mechanisms as selective sensitization? Relative frequency of occurrence or response probability is a general characteristic of verbal responses that should hold for any situation in which verbal responses are made. If the total variance in perceptual thresholds can be accounted for in terms of response probabilities, then the invocation of perceptual mechanisms may become unnecessary. On the other hand, if relative frequency of usage accounts for only part of the variation in thresholds as a function of personal value, then an argument can be made for the retention of explanatory principles that are specific to the perceptual situation.

To gauge the extent to which the effect of personal value reduces to the effect of differences in frequency of occurrence (response probability), Solomon and Howes repeated the original study of Postman, Bruner and McGinnes, controlling for word frequency within each of the six value areas by means of the Thorndike-Lorge word count (15). The average word frequency was approximately equal across value areas, but *within* each value area there was a group of frequent and a group of infrequent words. These words were presented for tachistoscopic recognition. The duration thresholds were then computed for frequent and infrequent words and for different value ranks. These are the main findings: (1) duration thresholds for frequent words are significantly lower than for infrequent words, (2) for frequent words there is little relationship between value rank and duration threshold although there is some slight trend in the expected direction, (3) for infrequent words the relation between value rank and duration threshold is

much more pronounced, and the difference between thresholds in the highest and lowest value areas reaches statistical significance.

It is clear, then, that relative frequency of occurrence in the English language is a most effective variable in the prediction of duration thresholds. There is also an interaction between the effect of word frequency and personal value. If words are very familiar, differences in value rank do not produce significant differences in perceptual sensitivity to these words. Their recognition is so fast, the threshold region is so small, that differences in subjects' predispositions have no opportunity to manifest themselves. On the other hand, when words are relatively unfamiliar, recognition builds up over a longer period of time and selective sensitivity to different value areas has a chance to show itself. It would appear that differences in word frequency can account for a considerable part in the variance of the thresholds. There does remain, however, some systematic variance that can be ascribed to selective sensitivity to the value areas represented by the words.

In their interpretation of the results, Solomon and Howes attempt to reduce the remaining differences between thresholds for high value and low value words also to effects of frequency. Different value ranks, they hold, probably represent *idiosyncratic frequencies* of word usage. Thus, a subject whose theoretical value is high on the Allport-Vernon profile is probably a person who uses theoretical words more frequently than a person whose theoretical value is low. High and low value ranks thus are equated to positive and negative deviations from the population norm in frequency of word usage.

We hesitate to agree with this reduction of the effects of value rank to ef-

fects of idiosyncratic frequency of word usage for several reasons.

(1) From the fact that word frequency accounts for a substantial part of the total variance in thresholds, it does not necessarily follow that this very factor explains the total variance. Such an hypothesis is legitimate but is still in need of testing before being used to explain the existing data.

(2) If we are to argue that very high and very low value ranks represent deviations from the population norm in frequency of usage, it would follow that the middle value ranks represent close conformity to the population norm. Solomon and Howes recognize this implication when they say that a "value rank of 3.5 would be considered to be representative of modal population word usage, since value ranks range from one to six" (13). On the basis of the threshold data they then quantify the degree of idiosyncrasy in word usage that corresponds to value ranks one and six: "... idiosyncratic word frequency, represented by an extreme (one or six) value rank, is equivalent to a maximum change amounting to 0.345 log frequency units, or 0.17 log frequency units above and below modal usage" (13). Considering the nature of the measuring instruments involved—the Thorndike-Lorge Semantic Count* on the one hand and the Allport-Vernon Study of Values on the other, we are somewhat doubtful about such quantitative relationships. It is unlikely that the word count provides more than a rough ordinal scale of the frequency of word usage by a sample of college students and similarly the Allport-Vernon value profile cannot give more than a rank order of value areas. How probable, then, is it that those for whom a given value has a rank of

3.5 use words in the area of that value in close conformity with the frequency of usage in the Thorndike-Lorge word count, and that value ranks one and six can then be set equal to idiosyncratic deviations from the population norm? Instead, we would prefer to say that the Semantic Count represents an approximate average rank order of population word frequencies which is correlated with average duration thresholds *in spite* of the fact that individual subjects' frequency of usage at any value rank is likely to deviate from the population norm. It is plausible, moreover, to assume that much of the variability around the population norm may be independent of those variables which lead to differences in Allport-Vernon profiles. Put somewhat differently, at each value rank individual variability in usage around the population norm may well be as large as variability between value ranks. In that case we should not be justified in treating the variables of value rank and idiosyncratic frequency of word usage as equivalent in the analysis of perceptual thresholds.

(3) On general theoretical grounds we are inclined to disagree with the reduction of the concept *personal value* to the concept *idiosyncratic frequency of word usage*. Let us assume, for the sake of the argument, that the relationship hypothesized by Solomon and Howes does hold: High value ranks do correspond to idiosyncratic word frequencies. We can then conclude either (a) what we have been calling personal values are merely variations in frequency of word usage or (b) one of the ways in which differences in personal values can manifest themselves is in differences of word usage.⁷ It all

* The Thorndike-Lorge counts (15) are based on the frequency with which various words occur in selected samples of publications in the English language.

⁷ Solomon and Howes recognize the possibility of such a formulation when they say that "emotional factors undoubtedly operate to an important extent in the *building* of word frequencies in a given life history" (13, p. 267).

depends on what it is we wish to do. If we merely want to predict duration thresholds for words, we can go a long way with the aid of the Semantic Word Count. If we want to put perceptual responses into broader contexts of motivated behavior, frequency of usage will not serve as a fundamental psychological concept. Frequency of occurrence is, after all, not a true psychological variable at all. We must always ask, "frequency of what psychological event?" It may, therefore, be more profitable theoretically to regard both frequency of word usage and duration thresholds as *dependent variables*—both manifestations of more fundamental psychological properties attributed to the organism, such as "habits," "hypotheses," or even perhaps "personal values." Whether the concept of personal value will for long survive the tests of theoretical parsimony and experimental utility is still doubtful, but it represents the type of psychological construct in relation to which both idiosyncratic frequency of word usage and duration of thresholds can be considered as dependent variables.

We would maintain, then, that variations in duration thresholds for words are not just a matter of different verbal responses having different probabilities of being emitted. To some extent at least, the thresholds reflect differential perceptual sensitivity rooted in lasting predispositions of the organism. What the subject *does* in the actual tachistoscopic situation is a resultant of his perceptual predispositions and of the strength of the verbal responses that are in his repertory. Both perceptual predispositions and verbal response strengths may, of course, turn out to be manifestations of more fundamental underlying principles.

What experimental steps should be taken next? We shall do well, first of all, to sample as widely as possible the relationship between frequency of word

usage, personal values, and perceptual thresholds, although the main trends have been firmly established by the work of Solomon and Howes. In order to put these facts into a broader setting of cognitive theory it then becomes important to explore the generality of the effects of personal value, word frequency and their interaction in other areas of cognition, such as memory. We shall then be able to see to what extent we are dealing here with peculiarly perceptual phenomena and to what extent with general principles of cognition. As the generality and stability of these factors are established, their integration in terms of more general and fundamental principles of cognition may become possible. The experiment to be reported in the following section represents an attempt in this direction.

THE EXPERIMENT

Purpose. The experiment is a repetition and extension of the studies of Postman, Bruner, and McGinnies (10) and Solomon and Howes (13). The purposes were (1) to obtain additional information on the relationship between personal values, frequency of usage and perceptual recognition thresholds for words; and (2) to investigate the relationship between personal values, frequency of word usage and memory. This extension of the experiment into memory was designed to test the generality of the principles found in the perceptual situation.

Procedure. The first part of the experiment was identical with that used by Postman, Bruner and McGinnies and Solomon and Howes.^a The procedure will, therefore, be summarized only briefly. A list of 36 words was constructed with each of the six value areas of the Allport-Vernon Study of Values represented by a group of six words.

^a The same apparatus and general procedure were also used by Howes and Solomon (4).

TABLE I
STIMULUS WORDS CLASSIFIED BY VALUE AREA AND FREQUENCY OF OCCURRENCE

	Value area					
	Theoretical	Economic	Political	Esthetic	Religious	Social
Relatively frequent	science knowledge truth	savings financial trade	leader citizen influence	orchestra artist music	faith religious spirit	society affection guest
(Mean log frequency = 2.56)						
Relatively infrequent	conception logic analysis	assets commerce efficiency	politician dominant status	graceful literature poetry	confession blessing divine	kindness loving hospitable
(Mean log frequency = 1.94)						

Classification of the words as falling into the various value areas was based on the consensus of three judges thoroughly familiar with the test. These words were presented for recognition in a Gerbrands (modified Dodge) tachistoscope. The duration threshold for each word was determined by the method of limits. Starting with very brief flashes, the experimenter increased the time of stimulus exposure until correct recognition had been achieved.

The factor of word frequency was controlled. The average frequency of word usage was equalized as closely as possible from value to value by means of the Thorndike-Lorge "L" count (15).⁹ Within each value area, a group of three relatively frequent and three relatively infrequent words was used. Thus, the effect of word frequency *per se*, of value rank *per se*, and the interaction of the two factors could be gauged. The mean log frequency for relatively familiar words was 2.56, for

relatively unfamiliar words was 1.94. The corresponding values in the experiment of Solomon and Howes were 2.23 and 0.60. Thus, both our frequent and infrequent words were more familiar than those in the other experiment. A list of the stimulus words appears in Table I.

The second part of the experiment, which represents extension of the investigation to memory, followed immediately after the determination of the recognition thresholds. Subjects were instructed to write down all the stimulus words which they could recall as having been presented to them in the tachistoscope.¹⁰ No time limit was enforced during the recall test. At the end of the experimental session the Allport-Vernon Study of Values was administered. Administration at the end of the experiment was advisable in order to keep the subjects in ignorance of the purpose of the experiment and to prevent them from adopting an explicit set towards value-related words. A total of

⁹ This particular count is the Lorge magazine count, giving the number of occurrences of a given word in a sample of approximately four and a half million words. This count is probably the one most applicable to a sample of college students.

¹⁰ Subjects were also asked to recall the guesses they had made prior to recognition. So few of these were recalled, however, that these data did not merit further consideration.

18 subjects—all college students—took part in the experiment.

Personal values, frequency and duration thresholds. Scores for each of the six value areas of the test were determined for each subject. On the basis of these scores, the six values were ranked from highest (rank 1) to lowest (rank 6). Average duration thresholds for both frequent and infrequent words in each of the value ranks were then computed. The results, which appear in Table II and in Fig. 1, suggest the following conclusions: (1) High frequency (relatively familiar) words are recognized more rapidly than low frequency (relatively unfamiliar) words. The average duration threshold for high frequency words is 0.109 sec.; for low frequency words, 0.118 sec. This relationship holds true for all value ranks *except value rank 1* for which low frequency words yield lower thresholds than do high frequency words. In general, then, our results confirm the findings of Solomon and Howes concerning the relation between word frequency and duration thresholds. (2) For high frequency words, there is no systematic relationship between value rank and duration thresholds. (3) For low frequency words, duration thresholds do vary systematically with value rank: the higher the value rank, the lower

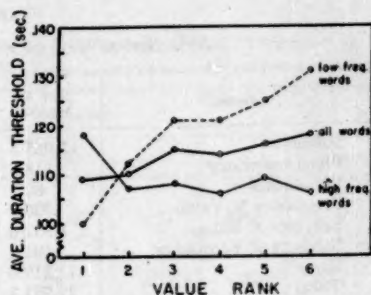


FIG. 1. Mean duration thresholds (in seconds) for frequent and infrequent words of different value ranks.

the mean duration threshold. The trend is pronounced and there are no reversals. Here again we are in substantial agreement with Solomon and Howes, although we obtain a sharper separation between the high frequency and the low frequency curves and a more clear-cut relationship between value ranks and thresholds for the unfamiliar words. (4) The composite curve (high and low frequency words combined) of duration thresholds plotted against value rank shows a gentle rise in thresholds with decrease in value rank. Whatever systematic trend this overall curve shows must be attributed to the trend in the low-frequency curve.

The statistical significance of these threshold data was tested by means of

TABLE II
MEAN DURATION THRESHOLDS (IN SECONDS) FOR FREQUENT AND INFREQUENT WORDS OF DIFFERENT VALUE RANKS

	Value rank						Means
	1	2	3	4	5	6	
Relatively frequent words	.118	.107	.108	.106	.109	.106	.109
Relatively infrequent words	.100	.112	.121	.121	.125	.131	.118
Means	.109	.110	.115	.114	.116	.118	.114

TABLE III
ANALYSIS OF VARIANCE OF DURATION THRESHOLDS

Source	Sum of squares	df.	Mean sum of squares	F	P
Subjects	13,965.5	17	821.50	32.13	<.01
Word frequency	148.3	1	148.30	5.80	<.01
Value rank	80.7	5	16.14	0.63	—
Frequency \times Value	300.7	5	60.14	2.35	.01-.05
Subjects \times Value	1,832.6	85	21.56	0.84	—
Subjects \times Frequency	404.4	17	23.79	0.93	—
Error*	13,219.6	517	25.57		
Total	29,951.8	647			

* Since the interaction, Frequency \times Value \times Subjects, was not significant, it was pooled with the Error term.

an analysis of variance which is summarized in Table III. We find that frequency of word usage is significant whereas value in and of itself is not. The interaction between frequency and value is, however, significant, i.e., the effect of value rank on duration thresholds depends on the level of word frequency. Value rank is a significant determinant of thresholds for low frequency words but not for high frequency words. The difference between the mean duration thresholds for words in value rank 1 and value rank 6 is significant at the 0.01 level for low frequency words and, of course, falls far short of significance for high frequency words.¹¹

As usual, we find subjects a significant source of variance, reflecting the

¹¹ The case of value rank 1 is of special interest. For high frequency words none of the differences between value ranks was significant, but it so happened that words in value rank 1 had the highest thresholds. For low frequency words, words in value rank 1 had significantly the lowest thresholds. As a result, the low frequency words of value rank 1 actually had a lower mean threshold than the high frequency words of the same rank. If the difference in frequency level had been greater, it is very doubtful that the two functions could have crossed in this manner. See the data of Solomon and Howes who used words that differed more in relative frequency than did ours.

wide spread of tachistoscopic acuities in an experimental group selected at random. Unlike Solomon and Howes, we did not obtain a significant interaction of subjects with frequency, i.e., subjects did not vary as regards the difference in thresholds between common and uncommon words. As in the other experiment, the interaction of value and subjects also falls short of significance. The effect of value rank did not vary significantly from subject to subject.

In general, then, our findings reaffirm that duration thresholds vary significantly as a function of both frequency and value rank. Whatever processes mediate the effect of value rank do not operate independently of the familiarity of the stimulus; they have an opportunity to manifest themselves if the recognition takes a certain amount of time to build up. Only when there is a more or less extended threshold region can selective predispositions come fully into play.

Personal values, frequency, and recall. Do personal values and relative frequency of usage have the same effect on memory as they have on duration thresholds? To what extent are the findings we have reported general principles of cognitive behavior and to what extent are they specific to perceptual

TABLE IV
MEAN NUMBER RECALLED OF FREQUENT AND INFREQUENT WORDS OF DIFFERENT
VALUE RANKS

	Value rank						Means
	1	2	3	4	5	6	
Relatively frequent words	1.33	1.06	0.78	0.89	0.94	0.94	0.99
Relatively infrequent words	1.28	0.50	0.50	0.72	0.95	0.89	0.81
Total	2.61	1.56	1.28	1.61	1.89	1.83	1.80

recognition? Table IV shows the results of the second part of our experiment in which the subjects' task was to recall as many of the stimulus words in the various value areas as they could. The same data are presented graphically in Fig. 2. We find that (1) more frequent than infrequent words are recalled, (2) there is a suggestion of a U-shaped relationship between value rank and recall, (3) the curve for infrequent words appears to have a steeper slope than the curve for frequent words, *i.e.*, value rank seems to make more of a difference in the recall of infrequent words than in the recall of frequent words.

The results of an analysis of variance of the recall data appear in Table V.

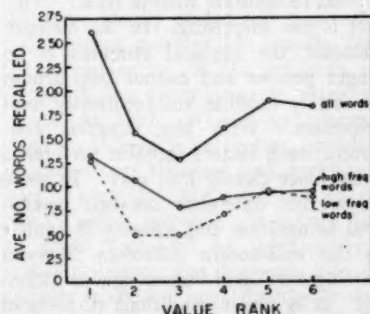


FIG. 2. Mean number recalled of frequent and infrequent words of different value ranks.

The only source of variance which reaches statistical significance is value rank.¹² Word frequency also gives a substantial F-value which, however, fails to reach the five per cent level. None of the interactions is significant. In the memory situation, then, value rank is an important determinant of recall. Word frequency operates in the expected direction but, at least for this sample, does not produce a statistically significant effect.¹³ Since the interaction, Frequency \times Value, is not significant, the apparent difference in the slopes of the high frequency and low frequency curves is not reliable. Neither of the two variables interacts significantly with subjects. Subjects, therefore, do not differ significantly in their response to either value or frequency.

The curves in Fig. 2 show a U-shaped relationship between value rank and

¹² Another study showing the effect of personal value on retention is that of McGinnies and Bowles (7).

¹³ The failure of frequency to be a significant source of variance is in part a function of the particular levels of familiarity used. If there had been a greater difference in familiarity between the relatively frequent and infrequent words, the effect of frequency would very probably have been greater. The important point to keep in mind, however, is that the effects of a constant difference in familiarity on perception and retention are being compared.

TABLE V
ANALYSIS OF VARIANCE OF RECALL DATA

Source	Sum of squares	df.	Mean sum of squares	F	P
Subjects	16.1	17	0.95	1.27	—
Word frequency	1.9	1	1.90	2.53	—
Value rank	9.4	5	1.88	2.51	.01-.05*
Frequency X Value	1.8	5	0.36	0.48	—
Subjects X Value	39.3	85	0.46	0.61	—
Subjects X Frequency	7.1	17	0.42	0.56	—
Error	64.2	85	0.75		
Total	139.8	215			

* Since none of the interactions is significant, they may be pooled with the original error, yielding a mean sum of squares of 0.60 (df. = 192). If this estimate of error is used, value rank becomes significant at the less than one per cent level. None of the other sources, however, reaches significance.

number of words recalled. To evaluate the significance of this trend, *t*-tests of the difference in mean numbers of words recalled were performed for all possible combinations of value ranks. The results of these tests appear in Table VI. Value rank 1 is significantly higher than all other value ranks as regards number of words recalled. The difference between value rank 3, for which the number of words recalled is smallest, and value ranks 5 and 6 is significant as well. Thus recall first drops and then rises again significantly as a function of value rank. A U-shaped relationship

between memory and such factors as hedonic tone (8) or attitude (11) has, of course, been reported before. The same trend is now shown to obtain for the memory of value-relevant words: words representing the most acceptable and the least acceptable values are both favored in memory.

Comparison of perceptual thresholds and recall. The effects of personal values and frequency on perceptual thresholds and on recall are in part parallel but also show important divergences. Personal values are a significant variable in both cases. As for frequency of word usage, it is a much more important determinant of response in perceptual recognition than in recall. This fact is not surprising. In the memory situation the physical stimulus is no longer present and cannot help arouse the highly familiar and frequently used responses. With less stimulus constraint, such factors as value preference come more clearly into play. In some ways, this difference between perceptual recognition and memory is similar to the well-known difference between "ambiguous" and "unambiguous" stimuli. It is under conditions of reduced stimulation ("ambiguous" stimuli) that directive factors have a maximum op-

TABLE VI
TESTS OF SIGNIFICANCE OF DIFFERENCES IN
MEAN NUMBER OF WORDS RECALLED FOR
ALL COMBINATIONS OF VALUE RANKS.
ENTRIES IN THE TABLE ARE
VALUES OF *t*

Value rank	Value rank					
	1	2	3	4	5	6
1		5.25*	6.65*	5.00*	3.60*	4.05*
2			1.40	0.25	1.65	1.35
3				1.75	3.05*	2.60*
4					1.90	0.95
5						0.35
6						

* Significant at the .01 level of confidence.

portunity to manifest themselves. The shift from perceptual recognition to memory similarly produces a reduction in stimulus constraint and hence brings the effects of selective predispositions into relief.

A consistent picture thus emerges. In the perceptual situation, the use of low frequency words slows down the recognition process and thereby affords an opportunity for such directive factors as personal values to influence the response. The shift from recognition to recall has a similar effect: It reduces the constraint exercised by the physical stimulus and thereby gives more "degrees of freedom" to the subject's response. The result is again a heavier weighting of directive factors in the determination of the response.

GENERAL CONCLUSIONS

(1) Our first general conclusion is that it will not be profitable at this stage of theoretical development to cast the laws of all cognitive processes into a uniform mold of stimulus-response correlations and response probabilities. When we have measured the relative frequency with which words occur in the English language, we have not exhausted the determinants of their perceptual recognition nor have we necessarily predicted their relative retention value in an actual memory situation. We have merely stated one parameter which enters, more or less heavily, into the determination of the subject's response in any given concrete situation. Hence we shall do well to continue thinking of laws of perception, memory, judgment and thinking and eventually laws of cognition, not merely of principles of verbal response. It is true that all these laws will be inferred from verbal responses, but they can also be usefully separated, operationally as well as conceptually. The principles of the logical reconstruction and of the empiri-

cal exploration of behavior need not be the same.

(2) Although general frequency of usage may be practically useful in predicting responses to verbal stimuli in a variety of situations, we do not believe that response probability is a basic psychological variable which will advance general cognitive theory. We would rather consider response probability at all times as a *dependent variable*, whether we are dealing with general frequency of occurrence in the language or with the subjects' responses in a specific situation. An empirical correlation between response probability and duration thresholds for verbal stimuli, for example, does not explain the duration thresholds at all. It merely poses the question as to the general psychological principles under which both the general and the specific response probabilities can be subsumed.

(3) We believe that directive factors such as the variable of personal values used in this research are useful in the development of a general theory of cognition. Their main function at this point is to emphasize the ways in which motivational and cognitive variables interlock in the analysis of behavior. The concept of personal value may well give way to concepts which are more basic psychologically and more readily manipulated experimentally. The fact will remain, however, that selective predispositions rooted in the organism's motivations play an important part in cognition.

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THE CONSTANCIES IN PERCEPTUAL THEORY¹

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I

We live and act in a world which we perceive as relatively stable in spite of the ever-changing impingements on our sense organs. This fact becomes a problem to those interested in evolving simplified conceptual explanatory systems. It has interested philosophers of all times and has troubled psychologists for the last several decades. Whatever may be the ultimate evaluation of psychological theorists, on one statement all can agree. It is the fact of perceptual constancy which makes effective behavior possible, from the simplest action to the most complex, from walking across the street to striving for a sane social order. Without some degree of constancy mere survival would be impossible.

As most commonly used in psychological literature, constancy refers to the similarity between specific apparent properties (such as the size, shape, or color) of two or more objects producing different proximal stimuli, with emphasis on the correspondence between these perceived properties and the actual properties of the objects. The objects need not be viewed simultaneously and indeed may be the same object viewed at different times. This fact leads directly to the continuous viewing of the same object, which is related to effects that can be termed continuity as opposed to constancy (27, p. 304). In actual experience, however, continuity is the rule, and constancy, as tradition-

ally investigated, merely represents a sample picked out for study from the more general experienced continuity. In this paper, therefore, any behavior which tends to preserve the continuity and stability of the perceived world in the face of ever-changing relationships between observer and environment will be labeled "constancy."

The perceptual constancies are studied experimentally by comparing the relevant characteristics of the percept with those of the object (5, 41). If perception remains constant as stimulation changes, then clearly there can be no constant relationship between stimulation and perception. The substitution of perceptual constancy for the rejected constancy hypothesis introduces a confusion of terms which, although deplored by some, may eventually be judged quite apposite. For Gestalt theory remains primarily concerned with stimulation-to-percept relationships and, in a sense, merely assumes a constant geometrical distortion in place of the constancy hypothesis, of which the law of *Prägnanz* can be seen as an elaboration. While such geometrical constancy is becoming seriously challenged by recent work on the role of subjective determinants in perception (*cf.* for example, 7), more functionally oriented psychologists, taking a cue from behaviorism, have shifted the focus of interest from perceptual constancy to object or thing constancy with all that this implies in terms of conceptual and experimental reorientation.

The study of constancy by comparisons of object with percept has not escaped methodological criticism. The frequently used constancy ratio (11,

¹ The author wishes to express his indebtedness to Adelbert Ames, Jr. and to Hadley Cantril for the theoretical orientation of this paper. For a more general, and more adequate, statement see especially Cantril (12).

34) offers the paradox of ratios greater than one, *i.e.*, over-constancy, which clearly must be considered as much an "error" as under-constancy. This objection has been met by the substitution of correlations (8), which give a more meaningful measure of the extent to which the organism is in functional rapport with its environment. The most serious limitation of such measures of correspondence is that they channel interest away from the means by which the organism achieves constancy toward a description of the final achievement. Far from being viewed as a limitation, however, this one-sided emphasis is hailed as crucial by those who would adhere strictly to the narrow definition of constancy in terms of percept and object comparisons, and who would, for example, rule out the Gestalt studies in constancy on the ground that they are "not properly concerned with 'distal' object relationships" (7, p. 58). It will be one of the primary arguments of this paper that the creation of such an artificial dichotomy makes the attainment of an adequate solution impossible. Constancy is functional only in so far as veridical distal relationships are established. (We shall return to this statement later in an effort to pluck the verbal plumage and reveal the behavioral meat beneath.) It is dangerous, however, to forget that such relationships are achievements of an acting organism, and that the means by which they are achieved are as much a part of the problem as are the relationships themselves. Constancy mechanisms and constancy achievements are inseparable. Any complete theory of perceptual constancy must encompass all its aspects.

II

A simple laboratory demonstration may serve to illustrate this point.² Let

² What follows is a description of one of a series of perceptual demonstrations designed

us photograph an ordinary playing card and reproduce it in three different sizes, one double-size, one normal-size, and one half-size. If we now view the double-sized card with one eye from a distance of, for example, eight feet, the card alone being illuminated in an otherwise dark room, it will appear to be only four feet from us and of normal size. Similarly, the half-sized card placed at four feet will appear to be of normal size and at a distance of eight feet, while the normal-sized card placed at six feet will indeed appear to be a normal card at a distance of six feet. We are thus confronted with three cards physically spaced from the observer in the order *small-normal-large* but seen in the reverse order, *large-normal-small*, and of physical sizes *small-normal-large* but seen in size as *normal-normal-normal*. Clearly by any definition of constancy calling for correspondence between perceived and objective properties this performance is the very antithesis of constancy behavior.

If we now make two slight changes in the above described configuration, the performance becomes quite different. In place of the half-sized card let us substitute a wrist-watch in a rectangular case of the same size and shape as the small card, and in place of the double-sized card substitute the cover of a pocket magazine which is of the same size and shape as the large card. This new arrangement, when viewed with one eye as described above, presents us with three objects in both physical and apparent order *watch-card-magazine*, and both physical and apparent sizes *watch-card-magazine*. This performance therefore is typical of perceptual constancy. These relationships are summarized in Table I.³

by Adelbert Ames, Jr. The author is grateful to Mr. Ames for permission to report this demonstration.

³ The description in the text, and as summarized in the table, is of the ideal perform-

TABLE I

APPARENT SIZES AND DISTANCES IN TWO MONOCULARLY VIEWED CONFIGURATIONS
(SIZE AND DISTANCE OF NORMAL PLAYING CARD TAKEN AS UNITY)

Condition I shows deviation from, and Condition II agreement with,
conventionally defined "constancy."

Test object	Visual angle	Actual size	Apparent size	Actual distance	Apparent distance
Condition I					
Half card	$\frac{3}{4}$	$\frac{3}{4}$	1	$\frac{3}{4}$	$1\frac{1}{2}$
Normal card	1	1	1	1	1
Double card	$1\frac{1}{2}$	2	1	$1\frac{1}{2}$	$\frac{3}{4}$
Condition II					
Wrist-watch	$\frac{3}{4}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{3}{4}$	$\frac{3}{4}$
Normal card	1	1	1	1	1
Magazine	$1\frac{1}{2}$	2	2	$1\frac{1}{2}$	$1\frac{1}{2}$

This demonstration raises many questions of relevance to perceptual theory (1, 20, 23, 24, 29, 33). We are here concerned only with its relationship to the problem of problemization in the study of perceptual constancy. With reference to a narrow definition of constancy, in terms of percept-to-object correlations, we must view as different the performances under the two conditions. In the one case we have constancy, in the other we do not. Similarly, as isolated responses to isolated stimuli from which behavior in the isolated situations may be predicted, they are different. Behavior in the one case will be successful and in the other unsuccessful. On the other hand, the two cases are certainly identical in terms of the perceptual processes involved. One cannot maintain that a constancy mechanism was operating in the one instance and not in the other. In order to avoid labeling the same per-

formance both constancy and non-constancy, we must seek a conceptualization which will encompass both aspects, *viz.*, the intra-organism constancy mechanism as well as the functional relationship between organism and environment. Such a conceptualization can be reached only by recognizing that object-to-percept and response-to-goal are not one-way roads. Neither are they completely isolated from each other, but rather represent two abstracted aspects of the same continuing process in which they are mutually affecting each other.

III

There have, of course, been many attempts to link these two aspects conceptually. Every investigator, no matter which isolated aspect may initially dominate his thinking, has eventually found it impossible completely to ignore the other. Two major lines of thought may be traced, although it is fully recognized that such an oversimplified presentation does violence to many views which cannot be classified so neatly. One line of approach is primarily concerned with perceptual mechanisms. It may call

upon the generalized configurational or field effects introduced by the Gestalt psychologists or upon specific mutually cancelling processes which leave the resultant perception unchanged. This latter type of explanation has been of recurring popularity since its early statement by Wheatstone, who noted that in his mirror stereoscope "the perceived magnitude of an object . . . diminishes as the inclination of the [optic] axes becomes greater, while the distance remains the same; and it increases, when the inclination of the axes remains the same, while the distance diminishes. When both these conditions vary inversely, as they do in ordinary vision when the distance of an object changes, the perceived magnitude remains the same" (38, p. 507). This quotation may be compared with more recent statements of the same type of explanation by, for example, Wallach (37), Schlosberg (32), and Gibson (18), as well as with the general Gestalt treatment of invariance (27). This approach has sought fixed laws of perception, usually natively or innately determined, and has considered the high degree of functional validity shown by perceptions to be fortuitous and fortunate. For example, to say that "luckily we are so made, and the world is so made, that under the normal conditions of life there exists in general a definite correspondence between our perceptions and the objects or the physical events which give rise to them" (30, p. 217) is but to state in its most extreme form a view which is representative of this approach (27, p. 305). The emphasis on the uniqueness of normal conditions holds throughout. Truth is to be had for the looking; seeing is believing, provided the conditions are auspicious.

The other major tradition specifically concerned with the constancies has followed a diametrically opposed orientation. It has consistently been impressed

with the very obvious need for, and attainment of, functionally useful responses. It has concentrated on the observation of such responses and the specification of the conditions which arouse them (6, 10). When questioned as to the intra-organism processes which mediate effective response to extra-organism factors, it either is not interested or is content to accept functional effectiveness as itself a sufficient explanatory principle. An unspecified evolutionary theory is implied here, and the fortuitous and fortunate implications are not missing.⁴

Attempts to account for the perceptual constancies along one or the other of these two general approaches have not been completely successful, nor have they met with universal acceptance, simply because neither approach deals with the whole problem. And attempts to pick and choose various parts from each in an effort to fit together the parts thus selected into some sort of jig-saw pattern, as advocates of eclecticism would have us do, can be adequate only if the initial artificial separation of the problem has not distorted it beyond recognition.

"Indeed!" said Mr. Pickwick; "I was not aware that that valuable work con-

⁴This common point of view, which sees one or another aspect of constancy phenomena as a more or less chance occurrence, is carried to a *reductio ad absurdum* in much recent work in which the attempt seems to be made to prove that perceptual constancy is achieved in spite of the organisms's best efforts to avoid it! By superimposing motivational or personality factors on unspecified perceptual processes, which are then removed from consideration after being mollified with some such high-sounding title as "autochthonous," this approach has concentrated on displaying distortions of perception. The resulting contradiction in terms, which finds motivational factors called upon to account for essentially non-functional behavior, is still further emphasized when perceptual distortion is used as an explanatory principle to account for the functional nature of perceiving.

tained any information respecting Chinese metaphysics."

"He read, sir," rejoined Pott, laying his hand on Mr. Pickwick's knee, and looking round with a smile of intellectual superiority, "he read for metaphysics under the letter M, and for China under the letter C, and combined his information, sir!" (16, p. 789.)

If instead, all phases of constancy behavior are treated as merely different aspects abstracted out of a unitary whole, no aspect of which would exist except for the whole, an adequate conceptualization seems possible (13). In this view functional effectiveness is mediated by the perceptual constancies, and constancy is in turn mediated by functional behavior. The stimulus-to-response phase of behavior has long been studied in psychology. This must now be supplemented by the parallel study of the effect of the response on the receptor system, *i.e.*, the response-to-stimulus sequence. Such a study may well lead to a reconsideration of the meaning of stimulus in psychological theory.

IV

Action and perception are inseparably related. An evaluation of the many theoretical attempts to link perception and action is a major study in itself and cannot be pursued in detail at this time. Suffice it to say that we are here referring neither to the mental acts of the Act Psychologists, nor to the kinesthetic cues which interested many of the early empiricists and functionalists, nor to the incipient motor responses of the motor theorists, nor to the overt, observable motor responses so dear to the behaviorists, although all these may properly be seen as among the historical antecedents of the position presented in this paper.

Rather, "action" as used here is more adequately defined in the homely words of the dictionary, "the doing of some-

thing." The perceiving of something and the doing of something are treated as two abstracted aspects of a continuing process of living, no one aspect of which can be understood without reference to the others. Dewey and Bentley have noted that the "differences between perception and manipulation seemed striking to the earlier stages of the development of psychology, but today's specialization of inquiry should not lose sight of their common behavioral status" (15, p. 299). And it is only in terms of this common behavioral status that such currently popular phrases as the "distal focusing of perception" take on any meaning. Reference to veridical distal relationships means nothing unless it means that perception and manipulation have been mutually consistent.

A clear recognition of the unity of perception and action enables us to achieve a conceptualization which does not rest on a meaningless *a priorism*, and at the same time avoids what Whitehead has termed the "Berkeleyan Dilemma," from which one readily descends to "a complete scepticism which was not in Berkeley's own thought."

"There are two types of answer to this sceptical descent. One is Dr. Johnson's. He stamped his foot on a paving-stone, and went on his way satisfied with its reality. . . . The other type of answer was first given by Kant. We must distinguish between the general way he set about constructing his answer to Hume, and the details of his system, which in many respects are highly disputable. The essential point of his method is the assumption that 'significance' is an essential element in concrete experience. The Berkeleyan dilemma starts with tacitly ignoring this aspect of experience, and thus with putting forward, as expressing experience, conceptions of it which have no relevance to fact. In the light of Kant's procedure, Johnson's answer falls into its place; it is the assertion that Berkeley has not correctly expounded what experience in fact is.

Berkeley himself insists that experience is significant, indeed three-quarters of his writings are devoted to enforcing this position. But Kant's position is the converse of Berkeley's, namely that significance is experience. . . . For Berkeley the significance is detachable from the experience"⁵ (39, pp. 11-12).

"What is significance?" Whitehead then asks. "Significance is the relatedness of things. To say that significance is experience, is to affirm that perceptual knowledge is nothing else than an apprehension of the relatedness of things." This relatedness of things, in the view presented in this paper, is revealed through action, or more precisely, action provides the concrete, operational definition of the relatedness of things, with reference to a particular space and time framework. Perception, then, is the product of the continual recording of the relatedness of things as defined by action. Perception is the apprehension of significance.

The psychological consequence of action, then, is a change of significance—change, because the relatedness of things is ever changing. The perceiver himself operates within the process and his very perceiving and acting constitute part of the relatedness of things. However, change is relative, and some significances are relatively stable and enduring, that is, have a high probability of recurring (9, 21, 35). Perceiving is, therefore, the apprehending of *probable* significances. It is predictive in function, or, as Ames has expressed it, perceptions are prognostic directives for action (1).

Out of the relatively stable significances, as determined by the relative

effectiveness of actions, a pattern of unconscious assumptions is built. These assumptions may be conceptualized variously as relatively stable ways of reacting, as patterns of probable significances, as value systems, or as concepts as to the nature of the objective world, which have been constructed through active participation in living, and may be considered as weighted averages of past experiences.⁶ The sum total of assumptions which the individual makes as to the nature and significances of the external world constitutes his assumptive world (12, 13, 23, 25). The assumptive world of any particular individual at any particular time determines his perceptions, that is, provides him with predictions of probable significances. His assumptive world is, therefore, in a very real sense, the only world which he knows. And since the assumptive world of each individual is to a certain extent unique, to that extent each one of us resembles Thurber's spy, who reported that what he had seen was "something very much like nothing anyone had seen before."

V

Such an approach makes possible a more adequate understanding of the perceptual constancies. Consider, for example, the constancies which are encountered in the study of depth perception. The most commonly cited case undoubtedly is size constancy, which, it is universally agreed, is dependent on the proper estimation of distance (5, 14, 18, 27, 36). Apparent size equals objective size only when apparent distance equals objective distance; or, in other words, size constancy and distance constancy are inseparably related, and

⁶ Since these weights are a function of the situation, no fixed hierarchy can be established apart from specific conditions.

⁵ And, we might add, for most psychologists who have sought an empiricist explanation. The most extreme example of detaching significance from experience is undoubtedly to be found in Titchener's context theory of meaning which set a fashion from which much of psychology has yet to emancipate itself.

both are essential for functional effectiveness.⁷

But apparent size is only one of the constancies dependent on apparent distance. Changes in many other important aspects of the perceived properties of objects are related to deviations from distance constancy, *i.e.*, to cases in which apparent distance does not correspond to actual distance. At least apparent size, shape, orientation in space and possibly brightness (1, 3, 18, 22, 34, 36) can be shown to be dependent on apparent distance. Apparent distance, however, is in turn dependent on size, shape, orientation in space, and brightness (1, 3, 17, 18, 23, 25) so that analogous statements can be made reversing the direction of the effect. If the apparent size, shape, orientation, and brightness of an object deviate from the objective size, shape, etc., then both the apparent absolute distance of the object and the apparent relative distances of various parts of the object will deviate from the corresponding objective distances (1, 2, 20, 23, 25). Apparent "thingness" and the apparent distance of the thing are complexly interdependent. There seems to be no reason for asserting the invariable primacy of one over the other.

There is, in fact, no aspect of depth perception which cannot be shown to be interdependent on all other aspects. There is no basis for asserting that some aspects are independent and some dependent, that some serve as cause and others as effect. Any apparent property

⁷ It is interesting to speculate as to why distance constancy is not included among the constancies commonly studied. Certainly the relative independence from specific proximal stimulation shown by apparent distance is at least as striking as that shown by size, for example. Size constancy, however, of all the constancies, has provoked the most interest simply because it astounds the most people. And this astonishment can probably be traced to an unacknowledged but binding conceptual link to the Lockean primary qualities.

can be altered in many ways: by varying impingements directly related to that aspect, by changing impingements related to other aspects which in turn are related to that aspect, or by keeping impingements constant and altering significances attributed to the impingements (20, 23, 25, 26, 33). The individual makes sense out of the intrinsically meaningless impingements by assessing their significance in terms of his assumptive world. He endeavors to create in the present a world which as closely as possible resembles his world of the past and which therefore gives him a feeling of surety that he can act effectively in the future. He does so not only because he wants to, but because he has no other alternative, this is the only world which he can know (*cf.* 2, 12).

It should be further pointed out that the effects described above, which apply to the perception of static objects, become immensely more complicated when movement is introduced. For example, the factor of relative movement (parallax) becomes important. Everything said above with respect to the static constancies applies in this case. Relative apparent distance is very strongly dependent on parallax indications (19). However, if apparent relative distances deviate from objective relative distances, parallax indications become perceptually related to apparent movements which deviate markedly from the corresponding objective movements (1, 2, 25). The number of apparent properties (in terms of which constancies can be specified) also increases to include such aspects as the speed and direction of movement. In addition, the relative importance of the static constancies changes. For example, static apparent size and shape can readily be altered by varying either apparent distance or objective size and shape, but when movement is introduced there is

a strong tendency to see the moving object as constant in shape and size (20, 23, 25, 26, 33). It may well be, therefore, as suggested earlier, that the static constancies are special cases derived from the more commonly experienced movement and continuity.

VI

A conceptualization of the perceptual constancies such as that outlined in the preceding sections can be extended into other areas of human behavior. We are able thereby to resolve the paradox which has found those psychologists most concerned with studying the functional effectiveness of man in his total environmental surroundings, *i.e.*, social, personality, and clinical psychologists, frequently neglecting the role of such supremely functional behavior as that represented by the constancies, although they are actively concerned with analogous problems variously conceptualized in terms of prejudice, stereotype, frame of reference, suppression, etc. The probable reason these psychologists have not found the conventional treatment of perceptual constancy to be of value in their disciplines lies in the definition of constancy in terms of correspondence between objective and apparent properties. Much of what might properly be labeled constancy-behavior in social and personality studies lies outside this definition, and indeed consists of maintaining a stable perceptual world which in one or more ways deviates markedly from the objective world. This contradiction disappears (and incidentally leaves room for a more adequate definition of the reality to which we are often told we must all adjust) when we define constancy-behavior as the attempt of the individual to create and maintain a world which deviates as little as possible from the world which he has experienced in the past, which is the only world he knows, and which

offers him the best possible chance of acting effectively and continuing to experience the particular satisfactions which he seeks out of living.

This view, incidentally, enables us to account for the maladaptive behavior evidenced by individuals whose perceptions quite grossly disagree with the objective situation. Such conditions are readily established in the perceptual laboratory: for example, one may cite the distorted room of Ames (1, 12), or the tilting room—tilting chair of Witkin (40). Some writers profess to see an element of stupidity in behavior in such situations (7, p. 59; 40, p. 40 f.). However, in the conceptualization presented in this paper, such errors, far from being stupid, represent the best possible and hence wisest attempt to interpret the perceptual situation in terms of the individual's assumptive world, with its host of weightings and probabilities relating the results of past actions to similar situations. To go contrary to the dictates of this accumulated experience, even though in any particular instance it may be wrong, would indeed be stupid.

VII

It may be fitting, in a paper devoted to constancy, to close with a few remarks on change. The conceptualization presented here has stressed the functional importance of constancy as well as the psychological mechanism by which constancy is achieved. Behavior will be random and ineffective unless it takes off from some relatively stable and determined foundation. Once the situation changes, however, in such a way that this foundation ceases to be the best possible one on which to base action, preserving it (*i.e.*, constancy) ceases to be of functional value. As outlined above, the consequence of action is a change in the individual's assumptive world, either reinforcing or modifying it. The consequence of con-

sistently ineffective action will therefore be an alteration of the assumptive world in the direction of a relatively stable, but changed, pattern of assumptions with resulting new constancies appropriate for effective action in the new situation. Paradoxical as it may seem, change is the mid-wife at the birth of constancy. As the world changes, so must we.

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THEORETICAL PSYCHOLOGY, 1950: AN OVERVIEW¹

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Since the end of World War II, psychology has been in a long and intensifying crisis. While the crisis has manifested itself in diverse forms, its core seems to be disaffection from the theory of the recent past. Never before has it seemed so evident that the development of a science is not an automatic forward movement, and that the *direction* of movement is a function of the plans of men. In consequence, men have been planning, and the prophetic pronouncement has become the standard prose form in recent literature.

The modest pursuit of logical analysis seems to have been submerged in the orgy of idol-smashing and tablet-delivering. Against this background the Dartmouth discussions on learning theory have seemed refreshing, in that an attempt was made to bring the tools of logical analysis to bear on the appraisal of our theoretical heritage of the recent past. Our central query was, "What is the status of learning theory?", but instead of looking for the *general* answer, we got down to cases. We raised *specific* questions about *specific* theories

and *specific* methodological issues. The decision to work in this way was sound. Our analyses can be made available in the open market of ideas for acceptance or rejection on their merits; they will not stand or fall by virtue of their linkage with any doctrinaire thesis (assuming that our group could have agreed on one).

Our case-orientation, however, requires supplementation in a number of directions. In my estimation the most urgent requirement is to set our findings on the condition of *learning* theory into the broad context provided by the recent history and current condition of psychological theory *in general*. I believe that if such a context is correctly defined, certain obvious directives for the development of theoretical psychology will be implicit in the definition.

Establishing the kind of historical perspective which seems required is, at best, a *geisteswissenschaftliche* enterprise. In this case the customary abandonment of the cultural historian must be compounded by lack of time for even the decent spelling out of intuitions. If, however, it is true that psychology is floundering in a protracted "moment of decision," there can be no excuse for a failure of nerve with respect to this line of speculation. I shall therefore crawl out on a very long limb by making a series of bald, and completely undefended, assertions about: (1) the immediate historical background of the present crisis in psychological theory, (2) the contours of that crisis, and (3) the implications of this cultural profile for the *tasks* or *functions* of theoretical psychology.

Needless to say my journey along this

¹ This paper was presented as part of a symposium given by the members of the Dartmouth Conference on Learning Theory, on September 7, 1950, at the Pennsylvania State College meeting of the American Psychological Association. Since the contributions to the symposium were made on the personal responsibility of each participant, it should not be assumed that the sentiments expressed in this paper represent, in any way, the views of the group. Acknowledgment, however, should be made to my colleagues of the Dartmouth Conference—William K. Estes, Kenneth MacCorquodale, Paul E. Meehl, Conrad G. Mueller, William N. Schoenfeld and William S. Verplanck—for the creation of an atmosphere congenial to the formulation and expression of these ideas.

fragile limb is an unaccompanied one. I assume personal responsibility for each wriggle along the way.

I. ANTECEDENTS OF CRISIS

During the two or three decades before the last war, psychology was in a relatively stable state in spite of conflicts. In this era, rigid lines of cleavage emerged among three diffuse classes of theory: "S-R," "field," and "psycho-analytic." As the era progressed, more detailed mapping would have disclosed a topography scaled to a number of dominating individual theorists within each class, their domains separated by "iron curtains" of formidable strength.

Certain generalizations about the "style" of this era are by now obvious. Among them are:

(1) That the central characteristic of this period was the attempt to develop comprehensive "theories" which, by the intention of each theorist, were at least potentially capable of ordering all of the data of psychology. If a theorist did not explicitly *claim* a universal range of application for his "theory," he did not, in any event, go out of his way to indicate which part of the universe he had in mind.

(2) That each of the contending "theoretical" structures was, in reality, an unevenly developed *program towards theory*, incomplete even at its most highly developed points, and characterized by a lopsided dependence on limited ranges of empirical material. It can be further contended that, since the limited inductive base of each "theory" was substantially different, much of the energy expended on theoretical polemic and "differential testing" was—*expendable*.

(3) That correlated with the overgenerality and empirical lopsidedness of such theoretical offerings was the tendency to blur the line between autism

and accomplishment, *e.g.*, a tendency to represent highly schematic models as finished theory; *illustrations* of how quantification might be *approached* as quantification; *guesses* about the potential fruitfulness of certain mathematical concepts as the *interpretation* of an entire mathematical discipline.

Some would regard generalizations of this order as a sweeping condemnation of our recent theoretical past. I want to make it very plain that I regard such a view as one-sided to the point of being callow. As measured against the complexity of our theoretical problems and the anti-theoretical bias widely held at the time, remarkable progress was made in the decades preceding the war. The airing of problems in the methodology of theory construction and the exploration of differing classes of variables for the analysis of behavior lifted us to a new level of sophistication in theoretical matters. It might even be maintained that the suggestion of Napoleonic over-ambitiousness, conveyed by our list of generalizations, is misleading. After all, frequent protestations of tentativeness may be found in the writings of the pre-war theorists.

Be all this as it may, a curious system of attitudes somehow got established among psychologists (at least those having theoretical interests) trained during that era. These attitudes are still widely held, and the extent to which they are retarding the progress of theoretical psychology is incalculable. Shuttlung, for the moment, from the role of cultural historian to that of depth psychologist, I come up with the following, as among the more conspicuous elements in this attitude-complex:

(1) The belief that psychology had already accumulated a sufficient number of cold, hard and basic "empirical facts" to *justify* attempts at comprehensive theory. The stage was set for

an era of Galilean construction, or Newtonian systematization, or, at the very least, Baconian descriptive organization.

(2) The belief that for areas where cold, hard facts were conspicuously missing, the situation could be rectified by founding theories on areas in which the cold, hard facts were presumably available, and then deriving consequences which would fill in the gaps.

(3) The belief that *typical* experimental situations for the study of a limited set of *representative* behaviors could be identified by a *a priori* analysis as the exclusive induction basis for a group of theoretical laws *adequate to all behavior*.

(4) The belief that theoretical programs were *theories*, that hypothetical guesses were *laws*, that anticipated consequences of hazily limned-in points of view were *theorems*.

(5) The belief that *quantification* of theoretical relationships, or their *co-ordination* to extant mathematical systems, was either immediately possible, or would automatically become so, without further methodological analysis.

(6) The belief that experimental data collected by members of one's own theoretical tribe could be trusted, while out-group data could not be trusted.

(7) The belief that rival theoretical positions could be given the decisive *coup de grace* by critical experiments.

(8) The belief that, at bottom, *any* theoretical assumption, no matter how foolhardy the inductive leap (assuming that there is something to leap *from*), is better than none at all.

(9) The belief that all will be well, provided one listens to his theoretical father-surrogate, or, more succinctly, *the departmental theory of truth*.

If there is anything to be rejected within the pre-war culture that I am trying to describe, it is this still tena-

cious, attitudinal—or, if you please, delusional—system.

II. CONTOURS OF CRISIS

Omitting grounds for all conclusions, and suppressing names and dates, my thumb-nail sketch of the present crisis would run as follows:

The current crisis has resulted mainly from a complex interaction between two factors: (1) *the stagnation, for roughly the past ten years, of the systems which dominated the pre-war scene; and* (2) *the demand for accelerated theoretical progress stemming from the marked increase in both the social recognition and the social responsibilities of applied psychology during the post-war period.* The ambitious programs of the thirties and early forties have either not been followed up, or, when prosecuted, they have not paid off. And never before has there been a situation in psychology in which the failure of theory to pay off could have been so public and strongly regretted a circumstance.

Against this background, a double-pronged re-evaluation of psychological theory and psychology in general has emerged. On the one hand, the technologists have discovered that extant theoretical formulations and experimental findings can give them very little of the rationale for the rapid technological progress that they desire. On the other hand, certain of the "fundamental" psychologists have become uneasy over the challenge coming not only from the technologists, but from the same social pressures which are driving the applied workers. The technologists have responded to the situation in one of two ways. Some have been led to believe, from whatever success their "rule of thumb" procedures have yielded, that technology can achieve full fruition without theoretical support. These workers have become confidently anti-theoretical. Others have nurtured the

belief that their applied discoveries are, in reality, the foundation material for the psychological theory of the future. Both of these positions lead, in practice, to the same consequence: the pre-emption of the entire domain of psychological science by the applied areas. The fundamental psychologists have also responded in two ways. They have either continued along in the pre-war grooves, but with somewhat reduced certitude and optimism, or they have recognized the stagnation of the pre-war systems, and have launched their own version of the theoretical wave of the future.

Most of the post-war remedies for the ills of psychology may be seen to bear an unfortunate resemblance to the disease which they were presumably designed to cure. If over-generalization of theory was a central defect of the pre-war era, it remains a central deficiency in the contributions of the current blazers of new trails. If, during the recent past, over-schematic and empirically hollow models were advanced in place of the genuine theoretical article, the hobby of model-making has really come into its own with the present reorienters of psychology.

III. THE FUNCTIONS OF THEORETICAL PSYCHOLOGY

By contrast with current panaceas, I can suggest only an arduous, perhaps painful, therapy, devoid of the customary magic bullets. We must start with the humiliating acknowledgment that *psychology is in a pre-theoretical stage*, and that the central problem of the fundamental psychologist is not what doctrine to embrace or concoct, but *simply to assay, realistically, how psychology can be made to move towards adequate theory*. As a corollary to this, we must recognize that knowledge of basic empirical relationships of the sort necessary for any high-order theory is pa-

thetically incomplete. Above all, we must reconcile ourselves to a long and tedious time-perspective. If psychology is at the stage of pre-Alexandrian physics, rather than in a Galilean or Maxwellian era, we might as well admit it; it is as honorable to work modestly *towards* such grand theoretical integrations as it is to constitute ourselves Galileos or Maxwells by fiat.

Accordingly I believe that progress towards adequate theory can be achieved only by substituting for the continued spinning of *psychological "theories,"* in the historically given senses of the term, the pursuit of *theoretical psychology* as defined by a set of modest objectives, geared to a realistic estimate of the status of our knowledge. Theoretical psychology, so defined, would prosecute five major tasks:

- (1) Education in the methodology and logic of science.
- (2) Analysis of methodological or "foundation" problems that are more or less unique to psychology.
- (3) Internal systematization of suggestive, but formally defective, theoretical formulations.
- (4) Intertranslation and differential analysis of conflicting theoretical formulations.
- (5) The construction of new theory.

I have time only to identify, in the most general way, the first four functions, and perhaps to ventilate, with a few ungrounded guesses, the vacuum that corresponds to "the construction of new theory."

(1) *Education in the methodology of science.* This is mainly an educational—in some ways, a promotional—job. But it is perhaps the most pressing *immediate* responsibility of theoretical psychology. There is a gap between what is known about the conditions of sound theory and what psychologists

know about these conditions. This gap must be closed. At the same time, we should recognize that knowledge about the methodology even of *physical science* is highly incomplete, and that physics is not psychology.

(2) *Analysis of methodological or foundation problems.* Such problems as the nature of psychological prediction, and related questions concerning the optimal characteristics of our units of causal and descriptive analysis; the relations between psychology and physiology; the roles of quantitative procedures, etc., must be given more detailed and productive analysis than they have yet received before we can rationally work towards adequate theory. The lesson of recent history, in this area, is the danger of rushing on to the construction of theory before clarification of such "metatheoretical" questions has been pushed to a level which might justify the effort. It is in this area also that the gap between psychology's recent methodological "coming of age" and true maturity is most evident. Too often there has been a tendency to confuse bright ideas with total illumination, and too many preliminary suggestions have rapidly congealed into unquestioned dogma.

(3) *Internal systematization.* If there is reason to suspect that a formally defective "theory" contains potentially fruitful empirical hypotheses or "insights," it becomes the responsibility of theoretical psychology to subject this material to "internal systematization." This involves the attempt to recast such formulations in as systematic, rigorous and precise a way as the given material, and relevant empirical knowledge, will permit. In order to exploit whatever has been achieved in *extant* theoretical formulations, internal systematization is indispensable. By itself, internal systematization cannot transform a programmatic scaffolding into finished the-

ory. Such work, however, is a necessary condition to the evaluation of what the theorist is saying; it is as useful for its revelation of gaps, logical elisions, limitations of empirical content, etc., as it is for the isolation and sharpening of any components which may prove of positive value. One hopes that internal systematization will become progressively less necessary as more satisfactory theoretical techniques come into general use. But this function of theoretical psychology will not terminate at any given date. The ability to state hypotheses in a formally impeccable way, and scientific imagination, are independent variables.

(4) *Intertranslation and differential analysis.* By "intertranslation" is meant the logical and semantic exploration of different theoretical language systems with the object of locating those areas of agreement which may be hidden behind different terminological facades. By "differential analysis," I mean the location of specific areas in which "theories" imply divergent consequences, and the execution of experiments designed to test them. It has not been sufficiently appreciated that internal systematization is often a necessary prelude to the proper exercise of this dual function. A further circumstance which has rendered attempts at intertranslation and differential analysis well-nigh futile to date has been the empirical lopsidedness and unevenness of development already pointed to as characteristic of extant "theory." Thus far, the coexistence of alternate theories which yield determinate derivations with respect to the same subject matter has been rare. Intertranslation and differential analysis will become progressively more important as theories generating consequences of comparable specificity with respect to the same empirical domain become available.

(5) *Construction of new theory.* Pur-

suit of tasks (2), (3) and (4) should lead to the sharpening, extension and convergence of extant theoretical programs, and thus to new theory. Indispensable as such work is, there is reason to believe that no amount of modification or realignment of historically given materials, will, by itself, take us far towards adequate theory.

If recent history does not mislead us, perhaps the most substantial *immediate* progress will come from undramatic attempts to develop narrow-scale theoretical formulations in which hypothetico-deductive methods are used for the detailed exploration and initial integration of restricted problem areas. It is pleasant to acknowledge that I am not alone in reading history in this way. But limited and concrete failures have no advantage over comprehensive and diffuse ones, and it seems necessary to urge that much thought be given to the long range objectives, and stage-by-stage strategy, of limited theory-making. A number of preliminary considerations *towards* the first phase of such a strategy seem called for by the status of current thinking about limited theory:

(1) The reaction to comprehensive theory could conceivably lead to undue enthusiasm for *any* formulation sufficiently limited in scope to appear concrete. This could easily result in the same kind of aridity that resulted, in an earlier day, from the fervor for *elegant* experimentation, regardless of the significance of the problem. Shrewd and daring speculation, no matter how programmatic, will always be a legitimate enterprise; indeed, miniature theory can only have miniature significance, if it does not take its direction from such conceptual mapping. If any law is to be passed, it should be against misrepresentation of the conceptual map as theory.

(2) Fruitful conditions for the construction of even the most "miniscule"

limited theories will not be realized until an honest estimate has been made of where we are in theoretical psychology. My diagnosis is that, even in the relatively developed field called "learning theory," we are still at the stage of a search for promising theoretical variables. It is unfortunate that the learning theorists of the recent past have felt impelled to elaborate advanced details and remote consequences of their "theories" *before* the potential fruitfulness of the variables identified by the theory had been carefully calculated, and their empirical applicability checked in a variety of limited contexts.

(3) In line with the foregoing, a major function of the limited theories of the future might well be the exploration of the possible fruits of tentatively identified theoretical variables. The variables employed should be selected with an eye to their potential adequacy to ranges of behavior outside the locus of systematization. More generally, this function of the limited theories of the immediate and intermediate future might be conceived as the performance of *methodologico-conceptual experiments*. Not only could different classes of theoretical variables be tested for their fruits, but—to take an example—the feasibility of various quantificational and mensurational procedures might be explored in this way.

(4) As measured against the state of our ignorance, the single most important function of limited theory would be its use, even in scattered areas, for the discovery of urgently needed empirical information. Areas in which such empirical exploration is most required are revealed by the conspicuous gaps in extant theoretical programs, particularly by gaps which prove, on analysis, to be common to the principal classes of theory as they now exist.

(5) Diverse narrow-scope systematizations of the same empirical domain

are to be encouraged. Differential success of such attempts, or differential tests of their consequences, could be of decisive significance in evaluating the possibilities for *extension* of the variables and methods employed. Depending on the outcome of such efforts, successively wider-scope formulations might be attempted.

(6) Any assumption that diverse limited theories, taking either the same or quite different domains as subject matter, will automatically get integrated is sanguine, to say the least. At present it would be irrational to predict what "size" of theory we can expect. Whatever integration we achieve will be purchased by repeated applications of the procedure of "intertranslation and differential analysis," by successive extension from a "core" theory, as described in (5), and, finally, by second-order attempts to arrive at more general postulates which would subsume the theorems of two or more limited theories.

(7) As a footnote to the preceding items, I might add that there is no reason to believe that we shall *ever* have unitary, comprehensive theory with an exhaustive range of application to all psychological phenomena. It is hard to understand the universality of this

belief—even as an element in the "pre-war delusional complex."

Having started with an indictment of the prophetic pronunciamiento, I have, surprisingly, proceeded to deliver one. But I have departed, slightly, from current fashion. No core-hypothesis for the psychology of the future has been generously proffered. I have not even said that the ultimate hope for psychological theory is the recognition of 23 fundamental kinds of everything. Nor have I shown how we can achieve unified social science theory before we have approximated theory in any one of the social sciences. The five tasks of theoretical psychology which we discriminated are already under way. It has not uniformly been recognized that resolutions of issues within *all five* areas are never final, and that the sound development of our science requires *continuous* prosecution of all of these tasks. Moreover, most of these tasks have so far been pursued casually and without systematic programming. Recognition of this, together with the substitution of organized for casual effort, will accomplish nothing dramatic—it will merely give us a fighting chance.

[MS. received October 20, 1950]

ON A STIMULUS-RESPONSE ANALYSIS OF INSIGHT IN PSYCHOTHERAPY¹

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Although a really extensive analysis of the concept of "insight" in psychotherapy and in clinical psychology is long overdue, no such ambitious task will be undertaken in this paper. The most casual examination of the treatment of insight by Alexander and French (1), Curran (3), McKinley (8), Rogers (11), Sears (12), Snyder (14), and Strecker, Ebaugh, and Ewalt (15) will reveal a multiplicity of shadings and nuances attached to the term. Nevertheless, the discussion here will be narrowly confined to the context laid out by Shaw (13), leaving for a separate treatment the larger question of a more general analysis.

In his thoughtful and challenging attempt to analyze some aspects of the psychotherapeutic process in terms of more primitive² concepts and to relate these to more general behavior theory, Shaw takes the position, essentially, that the symbolization in "consciousness" of the surrogates for the "repressed dangerous impulse" (i.e., the emission of highly symbolic responses which have an induction relationship to another set of responses which are at very low strength in consequence of the operation of an inhibitory potential) provides a hitherto unavailable and in-

accessible cue which the organism can utilize. The function of this cue is indicated as follows:

"The principle involved may be stated this way: If a given action has immediately rewarding consequences as well as more remote punishing consequences, such action, other things being equal, will be abandoned only when a stimulus to the action also becomes a cue to later punishment or non-rewarding consequences. When neurotic behavior is eliminated through psychotherapy, the stimulus which becomes a cue is a repressed motive. It is when insight is gained into this motive or when it is symbolized in consciousness (perceived) that it becomes a cue to the non-rewarding consequences of the neurotic behavior" (13, p. 39).

This formulation is based upon the study by Mowrer and Ullman (10), which presents a provocative analysis of the significance of the temporal factor in what they call "integrative behavior" (i.e., behavior of a non-neurotic character). In this study the animals were punished with shock if they failed to conform to a rat "etiquette" which required waiting three seconds after food-presentation before the food could legitimately be seized. It was found that none of the animals could learn the appropriate (i.e., non-punishing and therefore "integrative") behavior unless the buzzer was allowed to sound throughout the tabu period. The function of the buzzer, according to Mowrer and Ullman, was to provide a cue which would enable the animal to "bridge the gap," i.e., to "recognize" (so to speak) that waiting for the cessation of the buzzer would lead to non-

¹ This paper was read at the counseling and guidance session of the APA meetings at State College, Pennsylvania, in September, 1950.

² The word "primitive" is here used as it is by Whitehead and Russell (17) in *Principia Mathematica*. In this sense, if, e.g., the psychoanalytic concept of "identification" can be reduced to or derived from learning theory concepts in the system of Hull, Tolman, Guthrie or others, these concepts are by definition more primitive than the concept of identification.

punishing results, whereas failure to observe the "rule" would lead to food *but subsequently to shock*. This may be regarded as an elementary counterpart of foresight; that is, it is using signs to "bring the remote as well as the immediate consequences of a contemplated action into the psychological present and thereby compare and balance the probable (anticipated) rewards and punishments in a manner which enormously increases the chances that the resulting behavior will be integrative" (10, p. 87).

It is not clear from Shaw's analysis whether he intends his stimulus-response formulation to fit into a particular theoretical framework, such as Hull's or Guthrie's, or whether he sees it as eclectic. Since, however, he relies on the concept of reinforcement, it hardly fits Guthrie's system. Nor is it, in fact, consistent with a rigorous Hullian explanation. If anything, it would appear closer to Tolman, and this in spite of his use of reinforcement terminology. It might be said that this "symbolization in consciousness" permits the organism to form a field-expectancy to the effect that eating food under such-and-such conditions (*i.e.*, after cessation of the buzzer) will lead to such-and-such demanded situations (*i.e.*, food consumption without shock), whereas eating under so-and-so conditions (*i.e.*, while the buzzer is sounding) will lead to negatively demanded situations (*i.e.*, food consumption *with* shock).

The difficulty with such a formulation, however, and with Shaw's, is that it really fails to account for a very important aspect of psychotherapy. No one can practice psychotherapy for any length of time without observing the remarkable phenomenon which Freud (6) and Fenichel (5) have called "working through"; that is to say, the sequence of verbalizations and other behaviors

must be repeated again and again. Why should this be if, as Shaw conceives it, we are dealing primarily with a problem of the perception of relationships? Surely, one might argue, an organism as intelligent and rational as the human being would be able, once he had perceived the relationships, summarily to abandon his neurotic behaviors. The repetitive phenomenon would appear to be closer to experimental extinction of a non-insightful character, that is to say, experimental extinction which in no way requires the kind of cue which Shaw appears to demand as a prerequisite to the extinction of the neurotic responses. Moreover it seems appropriate to point out that Mowrer and Ullman were dealing with a *learning* situation; that is to say, with a situation involving the *acquisition* of a response, not the extinction of one.

Invoking the concept of extinction which does not require the relationship of what leads to what (and it seems clear from Tolman's [16] short treatment of extinction that he conceives of experimental extinction as essentially this kind of expectancy phenomenon) accords with the psychoanalytic formulations as to the nature of the therapeutic process. The neurotic behaviors, according to Fenichel (5), must be conceived of as *defenses*. "The motive for a pathogenic defense against instinct is always in the last analysis an estimate of the danger of an aroused instinct, the fear of the displeasure that would ensue if one were to yield to his instincts" (5, p. 16). In behavioral language, the symbolic surrogates are inhibited in much the same manner as were the motor responses in Estes' (4) animals under punishment. However, the anxiety generated has drive properties of its own, as has been demonstrated by Miller (9), and defenses (*i.e.*, a complex system of responses of both verbal and non-verbal character) are mobilized

to reduce the anxiety; it is these which constitute the pathogenic defenses. *And it is precisely because they are need relevant and drive reducing* that they tend to be perpetuated by the organism. They become the "resistances" which are so important in psychoanalytic theory. It is when these behaviors no longer perform their drive-reducing functions that their habit strength declines; and it is presumably one of the fundamental functions of analytic interpretation to render these behaviors non-reducing in this respect. At the same time, of course, the habit strength of new responses of a more integrative character is built up.

A further weakness to which Shaw's analysis is subject is that it appears to require that insight always and inevitably must occur *prior* to the elimination of non-integrative behavior, for extinction cannot occur until there is a cue pointing to negatively demanded behavior consequences. But is this actually the case? Alexander and French state that it is not. "In many cases it is not a matter of insight stimulating or forcing the patient to an emotional re-orientation, but rather one in which a very considerable preliminary emotional readjustment is necessary before insight is possible at all" (1, p. 127). This appears to make theoretical sense; it is when the anxiety response has begun to approach extinction and when new behaviors, uncued to anxiety, have begun to be emitted, that the ego is prepared to entertain "dangerous impulses" and insight can occur.

Citing an experiment by Culler, Finch, Girden, and Brogden (2) in which a conditioned response of paw-retraction was established in dogs and the response was subsequently extinguished, Shaw discusses the progress of extinction; or rather, he quotes a Hilgard and Marquis description: "Extinction can be secured only if the dog fails

to respond for some reason and thereby 'discovers' that the tone is no longer followed by shock" (13, p. 60). It is difficult to see how this is different from Tolman's non-reinforcement formulation; I do not think it is consistent with reinforcement theory, and I shall risk the prediction that none of the leading systematic reinforcement theorists (e.g., Spence, Hull, Skinner) would regard such "discovery" as a prerequisite to extinction. There is, in addition, a certain looseness of formulation in the phrase "for some reason." Actually, this looks like a fairly straightforward instance of type S conditioning; and for such extinction all that is required is continuous presentation of the CS without the US. Miller's (9) analysis contributes much to a clearer theoretical formulation here; in the study already cited he conceives of anxiety as an acquired drive, and makes the extinction of the drive-reducing behaviors contingent on the prior elimination of the anxiety. The problem and mystery in the human organism is just why the anxiety responses are not extinguished long before the client gets into the therapist's office. No satisfactory analysis has been presented of how this presumably learned anxiety is maintained.

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ONE KIND OF PERCEPTION: A REPLY TO PROFESSOR LUCHINS

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Dr. Luchins (10) has performed a valuable service by joining issue with the proponents of the so-called "New Look" approach to perceptual research. For he has asked many good questions, and in trying to reconstruct answers to them, he has shown the extent to which confusion reigns. This reply addresses itself to these questions. If some of the confusions are dispelled, its purpose will have been achieved.

There are two points to be got out of the way first. One concerns the unity of the "New Look" approach, which Professor Luchins rightly notes is not a "school." The term is descriptive in the sense that Modern Painting (with capital letters) is descriptive of everything on canvas since the post-Impressionists. Perhaps a Close Look indicates the futility of grouping a Chagall with a Klee. Let "New Look" merely denote some psychologists who are interested in studying the variance contributed to perceptual phenomena by factors reflecting the life history of the organism. The factors they investigate will vary; their mode of theorizing also varies. One cannot speak for all of them. For purposes of exposition in these pages, then, I shall inevitably reflect the "New Looker" whose work I know best—myself.

The second point has to do with Gestalt theory. Professor Luchins feels that it is under attack. He assures us first that Gestalt theory has shown the folly of trying to do what "New Lookers" are doing; besides, Gestalt theory has done it better and sooner. I for one do not wish to be drawn into a controversy on the merits of Gestalt work

in perception. I share with most of my co-workers in the field of perception a heavy debt to Köhler, Koffka, and Wertheimer. Their work represents the starting point of most contemporary research. My object is not to attack Gestalt theorists but to extend perceptual theory to phenomena generally underemphasized by them. Gestalt theory has means of handling variables like set, motive, value, and past experience—as Professor Luchins points out. But it has not exploited these means.

I shall approach the task of rejoinder from the point of view of a theory of cognition which holds that expectancy or hypothesis is a basic determinant of all cognitive activity—perception, memory, and thinking alike. As far as perception is concerned, my assumption is that expectancy has the effect of changing the availability of memory traces to communication with incoming stimulus processes. Since perception involves identification of objects, and since identification is dependent upon trace-process communication, I submit that memory traces always contribute to the organization of the field. My statement implies that the organization of the perceptual field is dependent upon identification of a percept.

Further, an expectancy leading to an alteration in the availability of memory traces and trace systems can be brought into being in many ways. Instructions, states of deprivation, past learning, cognitive beliefs—all these and many more factors may lead to the establishment of an expectancy. In sum, then, I shall be leaning in my discussion upon a

mechanism which mediates between "life history" variables and cognitive processes—expectancies whose function is to re-order the availability of traces.

Now let us proceed.

THE "ASPECTS" OF PERCEPTION

Half in parody, but only half, I have used the title "One Kind of Perception." Professor Luchins errs in a good cause when he reads into the recent perceptual literature a tendency to subdivide perceiving into different "aspects" or even kinds. "Do we know enough about perception to be able to so divide and compartmentalize it . . .?" He sees the following "aspects" as somehow being teased out of the total perceiving process by the "New Lookers": (a) Dynamic aspects, (b) Motivational aspects, (c) Defensive aspects, and (d) Functional aspects. Assorted authors are variously accused of speaking too loosely of these aspects or of accusing Gestalt psychology of not speaking of them enough.

There is indeed but one kind of perceiving, and the number of ways of considering it are as varied as the theoretical interests of psychologists. Professor Luchins is mistaken when he supposes that anybody is trying to "subdivide perception." The effort is to analyze it from various points of view. Frenkel-Brunswik (5) has drawn a distinction between "personality-centered" and "perception-centered" studies of perceiving. The distinction, while admittedly crude, provides a good point of departure. One may view perceiving as part of the repertory of responses through which the organism responds to the environment. He inquires how perceiving relates to other response systems. Surely there is nothing sinful in principle about inquiring into the relationship between perceiving on the one hand and, say, overall personality functioning (*e.g.*, introversion-extraversion)

on the other. Indeed, since perceiving is itself as much an aspect of personality as, say, striving, it is incumbent upon the student of perception to consider how perceiving is related to striving and to other components of personality.

But Professor Luchins is worried about the tendency of contemporary perceptual theory to subordinate its explanatory concepts to a kind of psychoanalytic dynamics. I concur with Luchins in this worry. Personality-theory "explanations" of perception—*e.g.*, that perceiving can be "explained" as a form of reality testing—are inevitably lacking in predictive value insofar as they fail to specify a mediating link between conventional personality processes and processes of perceiving. What Professor Luchins fails to see—and what is most critical to see at this point—is that many investigators are precisely concerned about such mediating mechanisms. The "hypothesis-information" theory presented by Postman (12) and myself (1) is one such attempt to account for *how* motives, past learnings, values, etc., can affect the perceptual recognition process: Klein (7), Hebb (6), Krech (8), and others have similarly been concerned with finding links. If current theories are poor—and they are not well developed yet—it is not through lack of trying.

I realize that when I talk about "mediating mechanisms" between, say, motivation and perception, I am doing violence to the integrated functioning of the organism. Wertheimer (14), Luchins (10), Krech (9), MacLeod (11), and others assure us, each for somewhat different reasons, that separation of behavior into perceptual, cognitive, affective, and motivational aspects is folly, and that the highest folly is the sort of "interactionist" theory which then seeks to put Humpty-Dumpty together again by invoking mediating mechanisms. These writers

prefer to take the whole behavioral package as the locus for their laws, insisting as Wertheimer (14) did that laws of psychological dynamics are the same for all aspects of behavior, or as Krech (9) does that we can never state adequate laws unless we state them for "perfinking"—a unit comprising perceiving, feeling, thinking, and motivational "aspects."

All of us, to be sure, would like to describe at one fell swoop the unity of the organism. The quarrel is not about ends, but about means. I for one feel severely hampered by the injunction not to separate perceptual, cognitive, and motivational variables. How shall I do my experiments? Between what variables shall I seek "if . . . then" relations? I shall have to continue separating and then putting back together my separated processes by hypothetical constructs if I wish to continue doing research. For the way of "separation-cum-interaction" is the only way of reaching firm conclusions about the unity of the organism. Analogies are misleading and foolish, but it is worth while to remember that in physics separation of processes seems to be a necessary precondition for the formulation of a more general theory. General theory comes not by *fat* but by successions of theoretical analyses and theoretical syntheses. When we have learned that A is a function of B, we may then proceed to learn that both are functions of C, etc.

Professor Luchins' remarks on the "defensive" aspects of perception require a word of explanation. Again, to say that perceiving serves a defensive function for the organism is not to "explain" the perception but rather to indicate a problem. If we find, as Eriksen and Lazarus report in a forthcoming paper (4), that neurotic patients independently judged by therapists to utilize repression as their major defense

have greater difficulty hearing sentences about sex and aggression than they do in hearing neutral sentences, while neurotic "intellectualizers" hear these loaded sentences more easily than neutral ones, we have located an important problem for the student of perception. From the point of view of an understanding of behavior in general, the finding has importance. Moreover, if one did not have a functionalist orientation to the problem of perception, very likely one would never have inquired about the relation between recognition threshold and defensive patterns. While the absence of such an inquiry might please some, to me it would be regrettable indeed. For the research has raised a serious problem about the mechanisms which inhibit the recognition process.

In the end we agree with Professor Luchins that one does not explain a phenomenon by stating the purpose which it serves. But certainly one locates problems by asking such questions. And it is to this end that I along with my fellow New Lookers will continue to ask questions about what perception does for the organism—or what thinking does, or what remembering does. When our explanations of behavior are complete, such questions will wither away just as surely as we no longer ask *why* the earth travels around the sun, but rather, how it does. The first wonder is about function.

THE DETERMINANTS OF PERCEPTION

Professor Luchins takes me and various of my colleagues severely to task for drawing a distinction between autochthonous and behavioral or structural and functional determinants of perception. *How right he is!* This is as mischievous a distinction as I know and I must admit ruefully to a part in increasing its currency.

The distinction has caused mischief insofar as it has made a conceptual separation as to what arises from the nature of the organism (or from the intrinsic nature of the brain field) on the one hand and what, on the other, is "contributed" by such "external," "functional," or "psychological" factors as set, past experience, etc. We may assume that all sides would agree that whatever operates to affect cognitive functioning operates in some common locus and that all factors operating in this final common locus have a common conceptual status. To give variables affecting perception separate status as we have done in the past is a mistake. Yet there are several points to be reckoned with in the distinction.

There are, first of all, certain *autochthonous limits* built into the nervous system. These are what Tolman might call "capacity laws." Thus, the nervous system can only operate within certain limits of stimulability, retentivity, rate of change, etc. The theorist and experimentalist alike must be vividly aware of these limits if he is not to become a public nuisance, for one too often finds studies reported and interpreted which show that, say, set "does not affect" some process which is already operating at an autochthonous limit. For example, a set to see a figure-on-a-ground may not affect the extent to which a highly simple and stable white figure is seen to stand out on a black amorphous ground (as measured, say, by a brightness match of the white figure). It may well be that under the stimulus conditions present, the process of figural segregation is operating at a maximum (an upper autochthonous limit) and only an act of God could push it further.

Another meaning of *autochthonous limits* is worth special note. Given a brain in a normal state or in a randomly varying state, and given an input stimu-

lus of certain characteristics communicating with given memory traces, the *limits of variability* that can be brought about by changing the set, attitude, or motive of the organism are fixed. The greater the amount of input stimulation in communication with trace systems, the less the perceptual variability that can be brought about by manipulation of set or motivation. The existence of such limits, to use old-fashioned functionalist language, guarantees that perceptual processes will not be at the complete mercy of passing sets, moods, needs, etc.

Bruner, Postman, and Rodrigues (3) have shown, for example, that as more and more univocal stimulation is "put into the brain field," the effect of set on constant error decreases. In this experiment the S has the task of matching a variable color wheel containing red and yellow segments to objects in the shape either of a banana or a lobster claw and so labelled. The objects in Condition I are colored a highly unstable orange-yellow induced by simultaneous color contrast. In Condition II the color is a well saturated orange-yellow paper figure. Figure and color wheel are separated by 90 degrees of visual arc in the first two conditions. In Condition III the well saturated orange-yellow paper figure is presented adjacent to the color wheel, the two of them against a homogeneous gray background, inside a viewing tunnel. The results are instructive. As one goes from Condition I to Condition II to Condition III, the amount of constant error decreases. Under the first condition, "red" labelled objects and "yellow" labelled objects give matches which differ by 58 degrees of red on our color wheel. This separation drops to 25 degrees in the second condition, and to a just discriminable 7 degrees under the third condition. As the "goodness" of stimulation increases the basis of com-

munication between stimulus process and trace improves, and constant error decreases. Determination of the perceptual field by old, established traces decreases as new trace systems are formed by unambiguous stimulus information.

What is left of the distinction between autochthonous and behavioral? Basically, we have reduced it to a statement of capacity. But what shall we say of such so-called "autochthonous" processes as closure, *prägnanz*, etc.? These processes, I would now hold, are not usefully described in terms of this distinction. Indeed, the process of closure, to take an example, is an abstraction based upon a sampling of various situations in which different expectancies have made different trace systems available for communication with input stimulus information. The task of the "New Looker" studying closure is not to "find" hypotheses or sets which are presumed to affect closure processes, but rather to *order* and *systematize* those variables which minimize and maximize the closure phenomenon. Bruner and Minturn (2), for example, have recently shown the way in which one can order and scale psychologically the extent to which various instructional sets (or hypotheses of trace-availability conditions) affect degree of perceptual closure. When we have scaled the total range of conditions determining the phenomenon, we will then and only then have a *general* law of closure. It is not a question of autochthonous and behavioral, but a question of ordering and systematizing conditions of variation.

PAST EXPERIENCE, ATTITUDES, NEEDS, ETC.

Little point is served by arguing whether Gestalt psychologists have dealt or can deal with the role of past experi-

ence, set, attitude, and need in perceiving. They *have* dealt with them; they obviously *can*. Professor Luchins is very insistent about this and he is correct. I would not argue the contrary.

But Professor Luchins does seem to imply that most other writers deal poorly with these variables. His main concern is that they invoke "past experience" or "attitude" as an *explanation* without recognizing that these are not processes. Indeed, it is precisely to handle the process mediating past experience (and attitude and need, etc.) that notions like hypothesis and trace availability are being tried out. If we assume that motives, past experience, attitudes, etc. have as their final common path the altered availability (and perhaps modified grouping) of trace systems, we have taken an easy step on the path toward rendering our statements about the conditions of the organism into process language. The step is an important one to take—whatever the nature of the mediating process one chooses to employ.

In another place, Professor Luchins appears to take strong exception to the kinds of explanatory processes invoked by various contemporary writers in interpreting their findings. He sets forth a rogues' gallery of concepts from Ego to *unbewusster Schluss*, sampling now from this writer and now from that. This is indeed a curious form of debate. Does Professor Luchins mean that psychology is very much divided in its preference for hypothetical constructs? If so, I will agree. If he means, on the other hand, that one should cease exploring new approaches to explanation, that one approach is already available in Gestalt theory or elsewhere, then I must conclude that his optimism is premature. At this point in our progress, the babble of many heterodoxies is rather a healthy noise.

EVALUATION OF LUCHINS' EVALUATION

My object in this section is to answer some of the criticisms of New Look research contained in the final section of Professor Luchins' paper.

First, he takes New Lookers to task for not living up to their promise to study perception in "real life situations." Real life, alas, is complicated for the experimenter, and one had best approach it well equipped with conceptual tools. Let us say that the object of most New Look research is to aid in the construction and verification of theory which will have as much applicability as possible to life outside the laboratory. I know this is a banal sentiment. Every theorist thinks he is doing just that.

We are also charged with paying too little attention to the situation in which our experiments occur: to instructions, the interpersonal setting, etc. This is a fair and a serious point. Certainly, the Postman and Bruner (13) study of perception under stress should warn us to proceed with caution. We need far better ways of describing the part of the stimulus situation not contained in our test patch or tachistoscope.

New Lookers are also taken to task for studying group trends in their data when they claim to be studying *The Perceiver*. I think Professor Luchins is right in the sense that we would do well to do some exploratory case studies of perceivers in the process of perceiving in different situations. The general point, however, is trivial. The tools of statistical analysis limit the New Look investigator as much as they do the next man.

Finally, Professor Luchins is worried about a reactive overemphasis on functional determinants at the expense of previously overemphasized stimulus determinants. Here I think that he is right. Perceptual functioning can only be systematized in terms of a "full information" theory—with reference to

stimuli and stimulus receivers and transformers. Such a theory is like a set of simultaneous equations, the terms in which can only be solved for by reference to the whole set of equations. These equations must perforce contain variables for stimulus factors, brain fields, the general state of the organism, and so on. Indeed, we all realize by now that stimuli cannot be described without reference to the organism, and that organisms cannot be described without account being taken of the stimuli operating upon them.

The question of theory is a good point on which to end, for it is here that Professor Luchins chooses to make his final plea. Let the concrete phenomena under investigation reveal what it is that should be studied about them, he says. He cites MacLeod to the effect that there be "first an attempt at an unbiased description of phenomena, then systematic experimentation designed to reveal the essential determinants of these phenomena, and finally revision of existing theory in the light of the new principles discovered" (11, pp. 196-197).

I do not think that concrete perceptual phenomena speak for themselves. Each theorist thinks that he has observed the "real" phenomena "naively." Professor Luchins feels that theoretically derived aspects and determinants of perception are "cluttering" and "confusing." A return to naive phenomenology (which Professor Luchins, like all good theorists, feels supports his own choice of data) will clear up all this nonsense. Let us look freshly and we shall discover principles in nature.

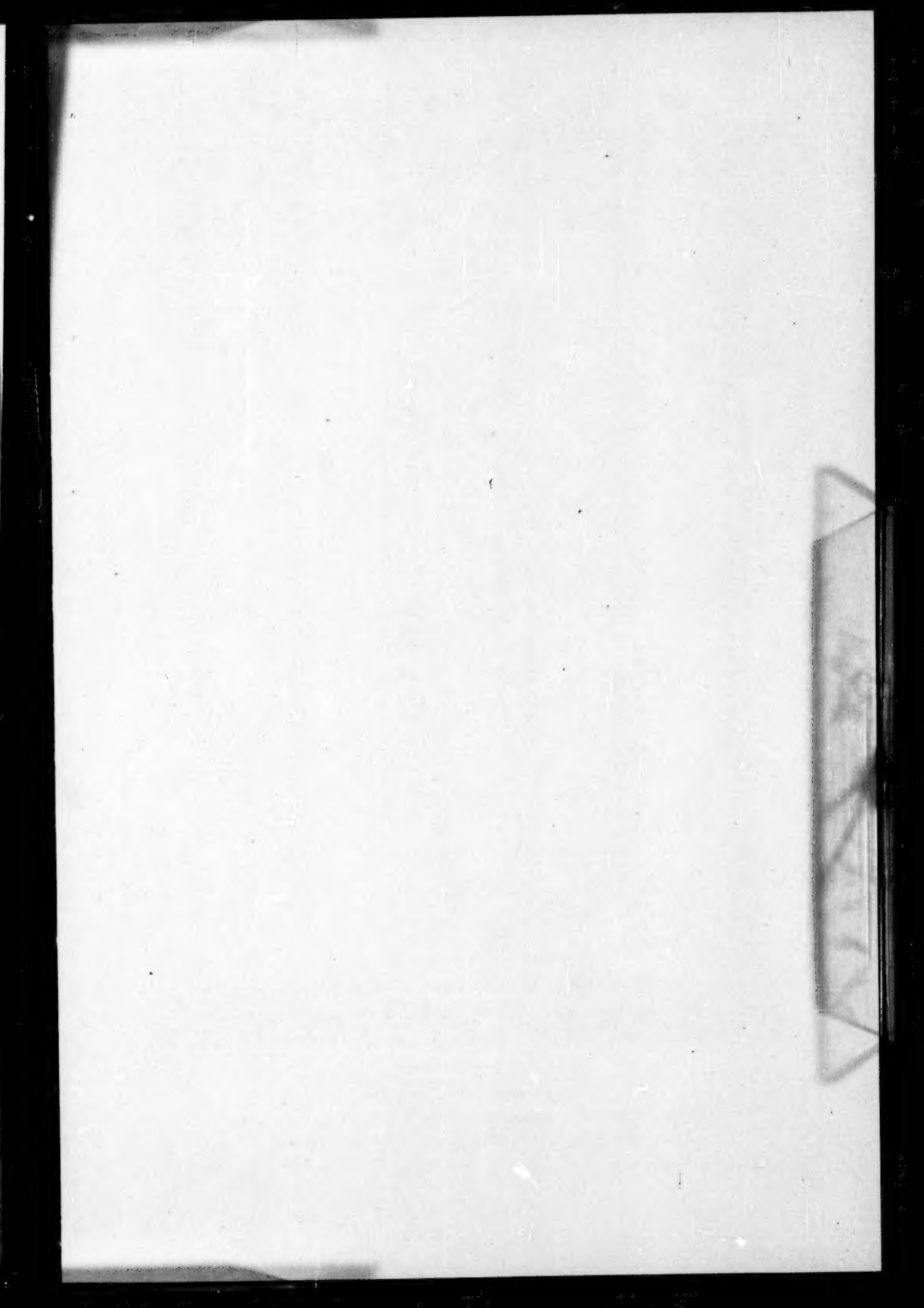
Alas, the data with which one chooses to work are always selected from a wide array of possible data—perceptual or otherwise. The basis of selection is theoretically derived. If Professor Luchins thinks it was otherwise for Gestalt theory, let him ask why it was

that Wertheimer decided to study apparent movement. Alas, too, principles are not discovered in nature by a good careful look at phenomena. They are *invented* and tested against selected data or concrete phenomena called for by one's theory. I suspect we need not less theory, but more theory—and better theory.

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1944	51	1	2	3	4	5	6	\$1.00	\$5.50
1945	52	1	2	3	4	5	6	\$1.00	\$5.50
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